The shadow disintermediation of risk-sensitive capital*

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Abstract

I investigate the risk implications of bank capital requirements in the presence of shadow banks. I show that in response to higher capital charges as an exogenous shock to loan retention, banks originate riskier loans. The probability of *ex post* borrower default rises by 8% (50% in relative terms). Defaults are absorbed by shadow banks. I uncover a novel pecking order of credit risk transfer with strategic adverse selection of banks vis-à-vis shadow banks. The riskier loans are sold to shadow banks with less monitoring expertise, but higher credit risk is not *ex ante* priced. I also find that the rise in default risk operates through lax screening *ex ante* as opposed to lax monitoring *ex post*. These findings collectively show that in the originate-to-distribute system, bank capital regulation distorts screening and triggers a credit risk transfer. While banks become safer, the credit risk originated by banks and borne by the shadow banking system is larger.

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1 Introduction

Regulators around the world responded to the financial crisis by imposing an overhaul of stringent bank capital requirements. The underlying view is that under-regulation is what leads to excessive bank risk-taking. However, there is evidence that banking regulations move financial intermediation into the shadow banking system. This generates the concern and controversy that disintermediation might counteract the main purpose of bank capital requirements that is to limit bank risk-taking. Despite such concerns, there is limited understanding of how bank capital requirements and shadow banks interact. In this paper, I study (i) how bank capital requirements affect bank lending and credit risk in the presence of shadow banks, and (ii) how credit risk migrates to shadow banks as a result of bank capital requirements.

Considering shadow banks in bank regulation is important because of their substantial role in the economy. Shadow banks (*e.g.* structured investment vehicles also known as CLOs, pension funds, hedge funds, mutual funds, corporations, private equity firms, and insurance companies) have dominated the U.S. syndicated loan markets since early 2000's. Through syndication and secondary loan markets, banks sold them a large portion of loans that they originated. Their investment concentrates in leveraged loans[1], broadly defined as loans to borrowers with high leverage and low credit quality. Ivashina and Sun (2011) report that their participation in this market increased more than 12 times from 2001 to 2007. According to Bloomberg, the total U.S. syndicated loan issuance in 2017 was about $2.5 trillion, of which more than half, about $1.5 trillion was classified as leveraged. In turn, $1.1 trillion of syndicated loans were classified as institutional, meaning that they were meant to be distributed to non-banks. Relatedly, shadow banks tripled their market share in the mortgage markets from 14% to 38% during 2007-2015 (Buchak *et. al.*, 2018). Hence, they are important players in private debt markets where banks traditionally originated loans and kept them on their balance sheets.

The main concern about shadow banks is the view that banks are special monitors. To the extent that bank monitoring is valuable, shadow banks might negatively impact the role of private debt to screen and monitor borrowers. The banking literature assumes that banks

[1] The definition of leveraged loan varies by institution. Dealscan defines a leveraged loan as any loan with a credit rating of BB+ or lower and any unrated loan. They are also called high-yield loans.

have a comparative monitoring advantage (*e.g.*, Leland and Pyle, 1977; Diamond, 1984; Ramakrishnam and Thakor, 1984; and Boyd and Prescott, 1986). Monitoring includes screening the creditworthiness of borrowers *ex ante*, and preventing them from behaving opportunistically *ex post*. Shadow banks, on the other hand, tend to have less information, expertise, and incentives than banks to monitor (Bord and Santos, 2011; Wang and Xia, 2014; Becker and Ivashina, 2016; and Billett *et. al.*, 2016). Consistent with this view, prior work links shadow banks to weaker underwriting standards in securitization (*e.g.*, Keys *et. al.*, 2010; Bord and Santos, 2011, and Wang and Xia, 2014). Both academics and practitioners also blame shadow banks for being partially responsible for the 2007 financial crisis. Ultimately, how bank capital requirements affect credit risk in the presence of shadow banks is an unanswered empirical question, one which I investigate in this paper.

Identifying the impact of bank capital requirements on credit risk is challenging because bank capital requirements are not random and counter-cyclical. They go up in downturns due to higher estimates in default risk. Furthermore, cyclical changes in the economy simultaneously impact shadow banks, and borrower demand. For example, in downturns banks lend less to risky firms in order to diversify their loan portfolios — while at the same time these firms deleverage due to poor investment opportunities. Thus, it is difficult to disentangle the effects of bank capital requirements from other changes that simultaneously affect bank lending and borrowers' default risk.

To investigate the impact of bank capital requirements on bank lending and credit risk in the presence of shadow banks, I study a natural experiment in the U.S. leveraged loan markets — a market segment where shadow banks are key players. The experiment is Basel II that provides an exogenous shock to capital charges associated with speculative-grade loans. Foreign banking laws require multinational banks to implement Basel II on a worldwide consolidated basis, and much earlier than the U.S. domestic banks. The foreign bank subsidiaries assign higher capital charges to speculative-grade loans, and regulatory capital is calculated as a fraction of total risk-weighted assets that are aggregated on a consolidated basis. I compare the speculative-grade loans originated by the U.S. subsidiaries of non-U.S. banks relative to U.S. domestic banks around the shock. This setting is suitable for identification because changes in capital charges are orthogonal to borrower fundamentals, shadow banking demand, and macroeconomic conditions. Exploiting this regulation as an exogenous shock to loan retention, I investigate changes in the credit risk of newly originated loans, and in the syndicate structure as a risk-transfer mechanism.

I first validate the empirical setting by showing evidence that banks subject to regulatory reforms find it costlier to originate speculative-grade loans. At the extensive margin, affected banks become less likely to originate loans with speculative-grade credit ratings after the reform. They also lose market share in this market segment. At the intensive margin, they sell larger portions of the new speculative-grade loans that they originate. The loan retention decreases by 7% (27% decrease relative to the mean). Furthermore, the probability that affected banks originate speculative-grade loans with non-bank lead arrangers after the reform increases by 12% (doubled relative to the mean).

Next, I investigate the impact of risk-sensitive capital regulation on banks' incentives to monitor borrowers' credit risk. To understand the relation with credit risk, I examine the *ex post* default of borrowers — controlling for observables at loan origination. My empirical approach is similar in spirit to Keys *et. al.* (2010) and Benmelech *et. al.* (2012). If the regulation leads banks to monitor less, I expect these loans to exhibit a higher probability of *ex post* borrower default. I hypothesize that the negative shock to loan retention will map to an increase in default risk. Consistent with this hypothesis, I find that under the new rules, new loans originated by affected banks are 8% more likely to be followed by borrower default within five years of loan origination compared to similar loans not subject to these rules[2] (50% increase relative to the mean). Consistent with shadow banks being less likely to compensate for the weaker monitoring by lead banks, I also show that the increase in defaults is confined to non-bank loans. In particular, loans perform the worst when they are originated with non-bank lead arrangers. I find no increase in defaults for loans originated by, and distributed within banking intermediaries.

I distinguish *ex ante* screening from *ex post* monitoring. In terms of the former, I show that banks subject to higher capital requirements extend loans to more indebted borrowers. However, the impact on default risk is large and positive even after controlling for observable sources of risk, including the *ex ante* indebtedness of the borrower. I then investigate whether banks are unaware of the risk that they originate because they exert less screening effort. To this end, I test whether defaults are higher on loans that are more likely to require banks to collect soft information. Instead, I find that loans extended to new borrowers (which I measure by the length of their relation), or loans originated by less informed banks (which I measure

[2]The typical leveraged loan in the U.S. has on average a 5 year maturity.

by the longevity of experience in the syndicated loan markets) do not perform differentially worse.

I further provide evidence of adverse selection between banks and shadow banks at loan origination. I document that defaults are stronger for loans originated with non-banks who have less monitoring expertise (which I measure by the average time of non-banks in the corporate loan market and borrower industry) and less private information on borrowers (which I proxy by the average length of relation and lack of bank affiliations). I also find that this higher credit risk is not *ex ante* priced. The analysis is agnostic about the efficiency of trades from the perspective of uninformed shadow banks. It is possible that the latter sell their stakes at a premium in the secondary loan markets and do not make a loss. Answering this question requires knowledge of trades and expected returns for each syndicate participant. However, no dataset that I am aware of contains this information. In the Appendix, I investigate potential explanations. I show that banks with high historical defaults become less likely to distribute loans to shadow banks in the secondary loan markets. This is consistent with the view that shadow banks learn, and that although risky loans are originated with uninformed shadow banks, adverse selection is broadly internalized in the secondary loan markets – reducing subsequent liquidity.

Finally, I investigate whether there is an increase in defaults because banks monitor less *ex post*, leading to risky behaviour on behalf of the borrowers. First, I search for direct evidence of lax monitoring through changes in covenants. Covenants give banks the ability to monitor borrowers by changing contract terms in default states, but they are costly in dispersed syndicates because they require the effort to renegotiate. I find that banks relax financial covenants, in particular those that restrict leverage. However, when I examine firm behaviour after loan origination, I do not find evidence of moral hazard. Firms use the debt financing that they receive to increase debt maturity and hoard cash. These results suggest that the increase in default risk is due to lax screening *ex ante* as opposed to lax monitoring *ex post*.

The central finding of this paper is that banks react to higher capital charges by reducing loan retention, originating riskier loans, and transferring credit risk to shadow banks. There is also evidence of strategic adverse selection between banks vis-à-vis shadow banks in the credit risk transfer. Borrowers under the new rules exhibit higher defaults (and bankruptcies) – but these defaults are internalized by shadow banks. I exploit information asymmetry and incentives between different parties within syndicates to disentangle the source of monitoring frictions: while incentives (stakes) — as opposed to information (screening) — drive the monitoring behaviour of banks, information (expertise) — as opposed to incentives (stakes) — is the driving force of the monitoring attitude of shadow banks in the credit risk transfer. Put differently, loans originated by banks with weaker incentives, and distributed to shadow banks with lesser information, exhibit more defaults under the new rules. Both channels shut down for bank participants. Although banks relax covenants, I do not find evidence of moral hazard on behalf of the borrowers.

This paper contributes to the literature studying the link between bank capital and shadow banks. The closest paper is Irani *et. al.* (2017). Using Basel III as an exogenous source of variation in regulatory bank capital, they find that weakly-capitalized banks reduce loan exposure in the secondary loan markets, and that shadow banks pick up the slack. They also show that this credit reallocation is linked to higher secondary loan market volatility. The novelty of this paper is the focus on credit risk. Using Basel II as an exogenous shock to loan retention, I show that banks originate riskier speculative-grade loans, and that they transfer them to shadow banks.

Second, this paper adds to the literature on risk-sensitive capital requirements. Prior work established a negative link between capital charges and bank lending in the European economy. Behn, Haselmann and Wachtel (2016) show that in response to an exogenous shock to credit risk in Germany, capital charges have gone up, and that banks reduced lending. Similarly, Fraisse, Lé, and Thesmar (2015) find that an increase in risk-sensitive capital requirements is associated with a decline in bank lending. In a related paper, Behn, Haselmann and Vig (2016) document that under the risk-sensitive capital design in Germany, risk weights are lower, and that default rates are higher. I contribute by studying risk-sensitive capital requirements in a market where shadow banks are key players. I show that these requirements lead to an increase in default risk, and a risk transfer to shadow banks.

Finally, this paper relates to the literature on shadow banks in the U.S. syndicated loan markets. Prior work investigated whether shadow banks distort banks' monitoring incentives during securitization. The evidence is mixed. For example, Benmelech, Dlugosz, and Ivashina (2012) show that banks do not select lower-quality loans for securitization. Similarly, Shivdasani and Wang (2011) do not find evidence that LBO deals funded by CLOs underperformed. However, Bord and Santos (2015) document that securitization is linked to higher default rates. Wang and Xia (2014), on the other hand, show that securitization distort banks' incentives to monitor borrowers *ex post*. The novelty of this paper is to investigate the interaction of shadow banks and bank capital requirements. I document strategic adverse selection between banks and shadow banks in the credit risk transfer.

The rest of the paper is organized as follows: Section 2 presents the experimental setting; hypotheses; identification strategy and data; Section 3 reports the results; and Section 4 concludes.

2 Empirical Design and Data

2.1 Experimental Setting

In this paper, I study Basel II as a natural experiment. The previous Basel I grouped assets into different risk categories, and assigned them a uniform capital charge. For example, all corporate loans had a capital charge of 100%. The total required capital was calculated as a fraction of risk-weighted assets. One concern with this framework was the incentive of banks to reach-for-yield. Basel II, published in June 2004, removed the uniform capital charge feature of Basel I. It imposed a risk-sensitive capital design, requiring banks to apply higher capital charges to riskier assets[3].

The Basel II feature that I exploit in this paper is the increase in capital charges for speculative-grade loans. Under the standardized approach (known as SA), capital charges are linked to external credit ratings, and those for speculative-grade loans are raised to 150% (Figure 1). Under the internal ratings-based approach (IRB) — where banks develop their own risk models – the relative capital charges for non-investment-grade loans also increase. The U.S. banks securitize non-investment-grade loans as a means of regulatory arbitrage, and securitization exposures require capital based on external credit ratings (Figure 2). Furthermore, multinational banks report benchmarking capital charges to external credit ratings where such ratings are available.

[Figure 1 here]

[Figure 2 here]

[3] See Basel Committee on Banking Supervision (1988, 2006) for the Basel agreements.

Exploiting Basel II as an exogenous shock to capital charges, I analyze the U.S. speculative-grade loans around the change. The setting has two features suitable for identification: it is exogenous and staggered. The U.S. subsidiaries of non-US multinational banks implemented Basel II because of home-country laws and much earlier than U.S domestic banks. Once the home regulator of a non-US bank implements Basel, the bank as a whole — including the US subsidiaries — calculate and report regulatory capital under the new standards. Hence, the US subsidiaries generate the risk-weighting parameters under Basel II. I collect information on the adoption of Basel-based laws from Central Banks' publications and press releases, and BIS annual progress reports. I also read SEC filings and annual reports of non-US banks to check the year where non-US multinational banks with U.S. subsidiaries are affected. Banks typically report preparing as soon as their home Central Bank announces the transposition of Basel into national law. I use these years as my event times in my empirical design. Table 1 reports the countries and years at which Central Banks announce the adoption of Basel, and Figure 3 presents the affected countries worldwide.

[Table 1 here]

[Figure 3 here]

The U.S. domestic banks implemented Basel II in 2014 – much later than the non-US banks – because it was immediately blocked. The U.S. regulators initially announced that they would require the largest and internationally active U.S. banks (*i.e.*, with assets exceeding $250 billion or foreign exposures exceeding $10 billion) to adopt Basel II advanced internal ratings-based approach (A-IRB), but this announcement generated competitive concerns. The main issue was that Basel gives unfair competitive advantage to largest U.S. banking organizations. The Quantitative Impact Study of Basel II (2005) showed that it would result in 15% drop in the amount of required regulatory capital for these banks. To address this concern, US regulators delayed the proposed rule-making until late 2007. Although the final rule was accepted in 2008, it included revisions which would prevent significant capital reductions, including a minimum three-year transition period (*i.e.*, parallel-run), and floors on the amount of capital reductions based on Basel I. By the time mandated banks started their transition process after 2011, a new Dodd-Frank provision mandated that the minimum capital requirement would be capped by what it would be under Basel I. While seven banks were approved to use the advanced approaches in 2014, three of them are still under the transition process. SEC filings suggest that the U.S. banks adopting Basel II do not expect a material impact. I therefore exclude the U.S. domestic banks from my analysis and perform robustness checks.

2.2 Hypothesis Development

The central hypothesis is that in response to higher capital charges as a shock to loan retention, speculative-grade borrowers become more likely to default. I derive this prediction from the financial intermediation literature predicting that the reduction in loan shares distorts banks' incentives to monitor (*i.e.*, to minimize a borrower's default risk). This literature argues that a lender has incentives to monitor borrowers if it has adequate exposure to their default (*e.g.*, Hölmstrolm and Tirole, 1997). The exposure to default is in turn provided by keeping loans on the balance sheets until maturity. Thus, banks have weaker incentives to screen and monitor their borrowers if they sell the loans that they originate (Gorton and Pennacchi, 1995). Furthermore, researchers argue that shadow banks could weaken banks' monitoring incentives because they are uninformed and have weak incentives (Parlour and Plantin, 2008; and Billett *et. al.*, 2016). I therefore also predict that defaults will increase more for loans that are sold to shadow banks, and in particular if the latter are uninformed or have little stakes.

The findings in this paper line up with the main argument of recent theories showing that higher capital requirements could increase risk in the presence of shadow banks. For example, Platin (2014) develops a model where banks circumvent capital regulation through shadow banks. The difference between formal and shadow banks in their model is adverse selection. Banks exploit shadow banks to offload their risky assets, but they cannot commit to not use their private information about these assets. They predict that a rise in capital requirements causes a surge in shadow banking activity at the cost of adverse selection. They also show that if banks face excessive capital requirements, and if adverse selection costs do not matter, banks increase risk and transfer it into shadow banks in a way that reduces welfare. Similarly, Harris, Opp, and Opp (2014) develop a model in which bank capital regulation constrains bank lending capacity, and shadow banks pick up the lack. They predict that competition with shadow banks could lead banks to make risky loans to stay profitable.

2.3 Empirical Strategy

This section describes the empirical strategy used to identify systematic changes in loan shares, syndicate structure, and default risk around the risk-sensitive capital shock.

2.3.1 Loan Retention

First, I analyze systematic changes in loan contracts around the risk-sensitive capital shock. Similar to Irani *et. al.* (2018), I hypothesize that higher capital charges lead banks to shrink loan retention to reduce their risk-weighted assets and increase their regulatory capital ratios[4]. Therefore, I predict a drop in lead shares for speculative-grade loans originated by affected banks compared to unaffected banks. Identifying this effect is a challenge because capital charges are not random. They increase in downturns due to higher estimates of default risk. However, downturns might require banks to hold larger shares because fewer investors invest in loan syndicates (Ivashina and Scharfstein, 2010), while portfolio diversification constraints promote loan sales. Thus, capital charges and loan shares suffer from endogeneity.

In the main identification strategy, I exploit the staggered implementation of risk-sensitive capital regulation by the U.S. subsidiaries of non-U.S. banks to separate the impact of capital requirements on loan shares from other spurious effects. The dependent variable is $Loan\ share_{ijt}$, which is a continuous variable that denotes the fraction that lead arranger commits to a facility. I estimate the following regression specification:

$$Loan\ Share_{ijt} = \alpha_j + \alpha_t + \delta \times event_{jt} + \gamma \times X_{ijt} + \epsilon_{ijt} \tag{1}$$

The dummy variable $event_{jt}$ is one in the post-regulation period after bank j becomes subject to risk-sensitive capital regulation. The parameter of interest δ measures the causal impact of risk-sensitive capital regulation on loan retention after relative to before the regulation, controlling for observable and unobservable differences between banks and within-banks over time. Thus, the identification compares the loan retention of the same lead bank after it switches to risk-sensitive capital. If banks react to higher capital charges by reducing loan retention, the coefficient δ will be strictly negative. The null hypothesis is that δ is zero, and that regulatory capital charges are unimportant for loan retention (*e.g.*, because banks can

[4]To increase regulatory capital, banks can either cut assets (*i.e.*, reduce lending or shrink loan retention), or raise capital (*i.e.*, raise new equity or reduce dividends in favour of retained earnings).

costlessly raise equity).

The unit of observation is the facility-loan level, arranged by lead bank j for firm i in period t. The lead bank fixed effect α_j controls for time-invariant bank characteristics such as size, funding structure, or bank risk management, and it absorbs average loan retention in facilities originated by lead bank j across all firms. Thus, it ensures that I capture changes in loan retention for the same bank. The time fixed effect α_t controls for time-specific trends, and X_{ijt} denote firm-level and bank-level controls provided by Dealscan at deal close — such as lender state, borrower state, parent country of the borrower firm, parent country of the bank, two-digit SIC borrower industry, and log(borrower sales). The state variables control for state-specific credit conditions, and industry variables capture industry-specific investment opportunities. When I include state-year and industry-year fixed effects to capture time-varying trends at the industry and state level, the results are similar. Country fixed effects such as the bank's parent country absorb the average loan retention for banks headquartered in the same country. To sharpen the analysis, I include deal purpose fixed effects for different deal categories such as investment, LBO, M&A, working capital, and refinancing. Generating a bias in δ requires a change unique to affected banks simultaneous with the shock that decreases loan shares for speculative-grade firms in the same industry, in the same state, and for the same purpose.

One concern for interpreting δ is that risk-sensitive capital might affect banks in ways other than capital charges. If banks experience a positive regulatory capital shock by the regulation, they will be willing to lend more and increase loan retention. This would lead to a downward bias in the estimation of δ. Additionally, if banks choose to diversify their loan portfolio in response to risk-sensitive capital, the estimate of δ would be biased upwards, but I would also observe a reduction in the shares for all loans. I therefore provide a placebo test by examining the impact of risk-sensitive capital on loans with no speculative-grade credit ratings (*i.e.*, for which capital charges do not increase). Furthermore, the staggered setting mitigates such concerns because potential biases are likely to cancel out in the cross-section.

To examine whether banks exploit shadow banks to reduce loan retention, I replace the dependent variable with $Migrate_{ijt}$, that takes the value of one for deals that are originated with a shadow bank. The empirical strategy is identical to the analysis regarding changes in loan retention, except that I do the estimation at the loan-package level. I assume a linear model and estimate the following regression specification:

$$Migrate_{ijt} = \alpha_j + \alpha_t + \delta \times event_{jt} + \gamma \times X_{ijt} + \epsilon_{ijt} \tag{2}$$

I predict an increase in shadow bank participation, and that the coefficient δ is strictly positive.

2.3.2 Default Risk

Data on *ex post* borrower default outcomes that I extract from Moody Default & Recovery Database allows me to identify the impact of higher capital charges on default risk. Let the dependent variable $default_{ijt}$ take the value of one if firm i experiences default *ex post* at any time within five years of taking out the loan. I estimate the following linear probability model at loan-level:

$$Default_{ijt} = \alpha_j + \alpha_t + \delta \times event_{jt} + \gamma \times X_{ijt} + \epsilon_{ijt} \tag{3}$$

The dummy variable $event_{jt}$ is one after bank j becomes subject to risk-sensitive capital regulation and zero otherwise. A reduction in loan shares should reduce the incentives of lead banks to monitor. If lead banks monitor less, I expect borrowers to become more likely to default. The parameter of interest δ measures the impact of the regulation on the probability of *ex post* default after relative to before the regulation, controlling for observables at the time of loan origination. A negative impact of the regulation on monitoring predicts a positive value of δ. The *ex post* borrower performance proxies for *ex ante* loan riskiness, which is unobservable[5]. In subsequent analysis, I attempt to disentangle *ex ante* screening from *ex post* monitoring. In terms of the former, I saturate Equation 3 by exploiting the asymmetry of information and incentives between different parties involved in syndication to understand the mechanism behind changes in default risk, and risk-sharing with shadow banks.

2.4 Data

I obtain my sample of speculative-grade leveraged loans from the Reuters Loan Pricing Corporation's (LPC) Dealscan database between 1997 and 2017. I consider a loan to be a

[5] Benmelech, Dlugosz and Ivashina (2012) investigate whether securitization is associated with risky lending by examining the performance of individual loans held by collateralized loan obligations.

"speculative-grade leveraged loan" if it has an S&P credit rating of BB+ or lower. This definition follows the Dealscan classification, and it is consistent with prior literature (Lim, Minton, and Weisbach, 2014; Nadauld and Weisbach, 2012; and Wang and Xia, 2014). My sample consists of dollar-denominated, and senior secured loans with floating interest-rate payments, made to non-financial U.S. public firms, and completed between 1997 and 2017 . Each observation corresponds to a specific loan facility – which I collapse to package-level to identify aggregate loan amount, syndicate structure (*e.g.*, number of lenders and shadow bank presence), and borrower performance. I use facility-level data when the dependent variable is allindrawn because different facilities are associated with different interest rates – and bank allocation – that corresponds to the amount that lead lenders commit to a given facility. Consistent with the literature, I additionally restrict my sample to the most common loan types (*i.e.*, Revolver, Revolver/Line, 364-Day Facility, Limited Line, Demand Loan or a term loan).

My primary focus is on the nexus of capital regulation and shadow banks, for which I use two alternative definitions based on (a) lender type (which I further classify by lender role), and (b) loan type. In terms of the former, I follow prior empirical work (see Lim, Minton, and Weisbach (2014), among others). I classify a lender as shadow bank if the lender is listed by Dealscan as neither a commercial or investment bank. Non-bank lenders include hedge funds, private equity funds, mutual funds, pension funds and endowments, distressed funds, collateralized loan obligations (CLOs), insurance companies, and finance companies. To identify commercial bank lenders, I include lenders whose type in Dealscan is designated as US Bank, African Bank, Asian-Pacific Bank, Foreign Bank, Eastern Europe/Russian Bank, Middle-Eastern Bank, or Thrift/S&L. Investment bank lenders are directly called as Investment Banks by Dealscan. Using this classification, I define two dummy variables: lead non-bank and tranche non-bank. The former takes a value of one if the loan syndicate has a non-bank lead arranger, and the latter takes one if the loan syndicate has a non-bank participant. To identify lead arrangers, I follow Standard & Poor's (2006) and LSTA (2006) definitions. I classify a lender as a lead arranger if it is assigned one of the following titles: administrative agent, agent, arranger, bookrunner, lead arranger, lead bank, or lead manager. Finally, I define a third non-bank variable based on loan type and call it an institutional loan. Institutional loan takes a value of one either if the original syndicate has a non-bank, or the loan has a Term Loan B (TLB).

I extract firm-level variables from Compustat. To do so, I match borrower's and/or borrower's parent name (borrowercompany) in Dealscan to Compustat (gvkey) and Execucomp (gvkey) by using the Dealscan and Compustat link file provided by Michael Roberts. Next, I manually match Dealscan to the Moody & Default Recovery Database by the borrower's and/or borrower's parent name in Dealscan (borrowercompany) to the name of the issuer in Moody's (issuer nam). Appendix A.1 descriptive statistics and Appendix A.2 provides variable definitions.

3 Results

3.1 Loan Sales and Migration

In this section, I investigate the impact of risk-sensitive capital regulation on loan retention, syndicate structure, and shadow bank presence.

3.1.1 Validation of the Empirical Setting

First, I validate the empirical setting by investigating the impact of the risk-sensitive capital regulation on the extensive margin of bank lending. If banks face higher capital charges for speculative-grade loans, they will have fewer incentives to originate such loans to lower their risk-weighted assets. This yields the following prediction: Banks subject to risk-sensitive capital become less likely to arrange deals for speculative-grade firms and lose market share in the leveraged loan market.

Table 2 presents the results on the extensive margin of lending. Specifically, I investigate the impact of risk-sensitive capital on the probability of originating a speculative-grade loan. In Column 1 to 2, the dependent variable is a dummy that takes the value of one if the lender is lead arranger within the syndicate, and zero otherwise. For this test, I use facility-lender level data, include one observation for each lender in a given facility, and include loan fixed effects. The identification design is similar in spirit to Khwaja and Mian (2008). Given that firms in the syndicated loan market typically borrow from multiple banks, I compare the probability of being a lead arranger to a firm between banks within a given syndicate at any given point in time. I find that the probability of being a lead arranger in a speculative-grade deal is 10.05 percentage points lower after the risk-sensitive capital regulation for affected banks relative to other banks (Column 1). However, I find no such change in the likelihood of assuming the lead arranger role for loans with no speculative-grade credit ratings (Column 2). This is consistent with speculative-grade loans bearing higher capital charges.

[Table 2 here]

In Column 3, I collapse deal-level data to monthly bank level, and compute the share of the number of speculative-grade deals originated by a lender in a month to the total number of speculative-grade deals originated over the same time period. I find that banks subject to the risk-sensitive capital regulation lose 1.33 percentage points market share per month as lead arrangers in the speculative-grade loan market relative to other banks. This result is economically meaningful since the average monthly market share is around 5%. I find no change in the market share for deals with no speculative-grade credit ratings (Column 4). This result confirms that after the regulation, speculative-grade loans bear higher capital charges.

3.1.2 Loan Sales

Although lead banks could originate speculative-grade loans to collect fees and sell them, they typically retain a portion on their balance sheets. They do this to signal loan quality to other market participants and increase liquidity. Furthermore, although banks typically originate and distribute term loans to non-banks in the secondary loan market, the little depth in the secondary market for credit lines requires them to hold the latter on their balance sheet (Bord and Santos, 2012). Hence, becoming a lead arranger requires some balance sheet exposure during the course of the loan. Thus, I predict the following. Conditional on originating a speculative-grade loan, banks subject to risk-sensitive capital regulation reduce loan retention.

[Table 3 here]

Table 3 estimates Equation 1 and reports the results on the intensive margin of lending to speculative-grade borrowers. In Column 1, I show that after risk-sensitive capital, lead banks reduce their shares in speculative-grade facilities 7.04 percentage points[6] (Column 1). However, I find no such change in the lead shares in loan facilities with no speculative-grade

[6]Using the Shared National Credit database (a supervisory credit register administered by the Board of Governors of the Federal Reserve System, the Federal Deposit Insurance Corporation and the Office of the Comptroller of the Currency), Bord and Santos (2012) investigate the time-series evolution of the individual

ratings (Column 2). These results are consistent with speculative-grade loans bearing higher capital charges, and suggest that banks shrink loan retention in loans with higher capital requirements. This is consistent with Irani *et. al.* (2018), who also find that low-capital banks shrink loan retention through loan sales in the secondary loan markets to improve their regulatory capital position.

To understand what drives the lead share drop in speculative-grade loans, I investigate whether lead arrangers form more diffuse syndicates, and distribute loans to shadow banks. In Column 2, I replace the dependent variable in Equation 3 with the natural logarithm of total number of lenders in loan syndicates. The total number of syndicate participants in speculative-grade loans is 24.21% higher when lead bank becomes subject to the risk-sensitive capital regulation. This suggests that banks form more diffuse syndicates – sell the loans that they originate to a greater number of participants – to keep smaller portions of them. Next, I test whether higher capital charges lead to a migration of credit origination to shadow banks. In Column 3, I estimate Equation 2 and investigate the probability that the loan has a non-bank lead arranger. I find that lead banks 11.36 percentage points more likely to co-originate loans with non-bank institutional investors after the risk-sensitive capital regulation.

In addition to loan retention, the increase in capital charges might also affect the maturity and price of lending (the interest rate charged on loans). I report these tests in Table 4. While capital charges for loans under the standardized approach are automatically assigned at loan origination and do not change, capital charges for loans under IRB are continuously updated based on internal credit risk estimates, implying an increase in capital charges in economic downturns. Berger and Udell (1988) argue that banks shorten debt maturity to renegotiate loan terms more frequently. As most international foreign banks active in the syndicated loan markets adopted the IRB approach, they might find the option to frequently renegotiate loan terms more valuable. Thus, I expect that banks subject to risk-sensitive capital might shorten debt maturity to be able to renegotiate more frequently. In line with this conjecture, the result in Table 4, Column 1 shows that after the regulation, lead banks shorten debt maturity

share of each credit that lead banks originate and retain on their balance sheets in the syndicated loan markets. They find that in the years after origination, lead arrangers tend to increasingly reduce their aggregate exposure to the loans that they originate. The estimates in Table 2 reflect the impact of risk-sensitive capital on lead shares at loan origination; therefore, they are likely to be a lower bound on the reduction in loan retention. As Dealscan only provides loan-level data at loan origination, and as I have no access to the SNC database, I cannot verify this conjecture.

by 10.34% (which roughly corresponds to six months on average). The decrease in maturity is concentrated to bank tranches (Column 2), and the change in the maturity of non-bank tranches is negative but statistically insignificant (Column 3).

[Table 4 here]

In Column 4, I replace the dependent variable by allindrawn, which corresponds to the interest rate that borrowers pay in basis points over LIBOR for each dollar drawn down, including the fees. The coefficient is positive, indicating that, relatively, interest rates increase, but the effect is very small and statistically insignificant. This suggests that banks respond to the risk-sensitive capital regulation by reducing loan retention and loan maturity, instead of increasing interest rates. This result is consistent with the prior literature that found that banks react to higher capital charges by reducing loan quantities instead of increasing interest rates (Behn, Haselmann, and Wachtel, 2016; and Fraisse, Lé and Thesmar, 2017).

To sharpen the evidence that risk-sensitive capital leads to reduced loan retention among speculative-grade loans with higher capital charges, I depict the average loan share of lead banks in speculative-grade facilities around risk-sensitive capital in Figure 4. I observe a sharp drop in the lead share of treated banks subsequent to regulation on average from 27 to 16%. In addition, I find no evidence of pre-trends. Figure 5 depicts the kernel density plots showing the loan share of lead banks in speculative-grade facilities before risk-sensitive capital (grey line) and after risk-sensitive capital (black line). There is a clear leftward shift in the distribution of lead shares after the regulation. This graphical evidence suggests that the regulation is negatively correlated with lead share in speculative-grade loans, and that the results in Table 3 are not driven by outliers.

[Figure 4 here]

[Figure 5 here]

Similarly, I plot the dynamics of the probability of having a non-bank lead arranger in speculative-grade loans around risk-sensitive capital in Figure 6. I observe a sharp rise in the probability of non-bank lead arranger on average from 5% to 20%.

[Figure 6 here]

3.2 Default Risk

In this section, I study whether in response to higher capital charges as an exogenous shock to loan retention, banks have weaker monitoring incentives and originate riskier loans. Thus, I hypothesize that borrowers subject to higher capital charges default systematically more *ex post* after the regulation. The reasoning for defining monitoring incentives based on lead shares is the idea that the wealth effects of credit risk are ultimately born by lead banks in proportion to their percentage ownership (*i.e.*, loan shares). Thus, loan sales reduce the incentives of banks to conduct credit analysis and monitor borrowers (Gorton and Pennacchi, 1995).

I define $default_{ijt}$ as a dummy variable that takes one if firm i defaults at any time within five years of taking out the loan. I define $bankrupt_{ijt}$ as a variable that is one if the borrower goes bankrupt at any time within five years of taking out a loan. Figure 7 displays the average market-adjusted probability of *ex post* borrower default outcomes for speculative-grade contracts originated by the U.S. subsidiaries of foreign banks that become subject to the risk-sensitive capital regulation[7]. Before the regulation, borrower performance is similar to that of other borrowers in the syndicated loan market. However, after the regulation, there is a significant increase in *ex post* borrower default outcomes for speculative-grade loans originated by treated banks, compared to all other loans in the syndicated loan market.

[Figure 7 here]

Table 5 displays the results from estimating the linear probability model in Equation 3. The first two columns include all loans given to the speculative-grade public firms in Dealscan. Speculative-grade loans originated by affected banks are 8.28 percentage points more likely to be followed by *ex post* borrower default after the risk-sensitive capital regulation, relative to other loans (Column 1). Replacing the $default_{ijt}$ dependent variable with $bankrupt_{ijt}$ slightly reduces the effect to 7.24 percentage points (Column 2). Consistent with no increase in capital charges being associated with no monitoring distortions, there is no increase in *ex post* borrower

[7]To calculate the market-adjusted probability of *ex post* borrower default outcomes, I de-mean the *ex post* borrower default variable with the average *ex post* borrower default probability in the syndicated loan market. To compute the % of default outcomes on the y-axis, I align contracts around the year of risk-sensitive capital regulation and calculate the average *ex post* borrower default for all contracts within the same distance to the regulation. Because the regulation is staggered, there are lesser performance data points available for banks that are subsequently affected. For that reason I compute cumulative annual averages within the same distance to the event.

defaults for the sub-sample of loans extended to non-speculative grade public firms. Instead, non-speculative-grade borrowers of affected banks default systematically less *ex post* (Column 3), and go bankrupt systematically less *ex post* (Column 4). There is no change in default probability for the entire sample. Taken together, the results in Table 5 suggest that higher capital charges map to higher credit risk. This is consistent with the hypothesis that in response to a negative shock to loan retention, banks have fewer incentives to monitor default risk.

[Table 5 here]

I re-estimate Equation 3 with alternative default windows, and report the results in Figure 8. I find that the increase in default risk essentially appears in the long-run — essentially after four years of loan origination.

[Figure 8 here]

Next, I analyze the mechanism through which default risk rises under the new rules. Default risk might rise because borrowers are not properly screened *ex ante*, but also because of lax monitoring *ex post*. In the next two sections, I attempt to disentangle *ex ante* screening from *ex post* monitoring. In Section 3.3, I investigate changes in borrower characteristics, and exploit the asymmetry of information and incentives between lenders at loan origination as evidence of lax screening. To test whether banks monitor less *ex post*, I analyze the strictness of covenants (Section 3.4.1), and I examine borrowers' characteristics after loan origination (Section 3.4.2).

3.3 Does default risk rise due to lax screening *ex ante*?

3.3.1 Riskiness at loan origination

I first test for lax screening and look at borrower characteristics at loan origination. I show that the borrowers of affected banks are more indebted and observably riskier. I use Equation 3, and I replace the dependent variable with the debt to assets ratio of the borrower one year prior to loan origination. I also saturate Equation 3 with industry-year and state-year fixed effects. In doing so, I take into account the unobservable economic trends that could differentially correlate with borrower characteristics of banks subject to the regulation. The result is shown in Table 6. I show that the debt to assets of borrowers of affected banks increases by 4.37 percentage points. Thus, banks subject to the regulation lend to borrowers that become observably riskier.

[Table 6 here]

Next, I look at the impact of the regulation on *ex post* defaults, taking into account time trends in industry, state, and size, as well as observable contract characteristics (such as loan amount, loan maturity, and interest rate) and *ex ante* borrower indebtedness. The results are collected in Table 7. When I include industry-year, size-year and state-year fixed effects, the estimate becomes somewhat larger at 9.14 (Column 4). It is still positive and significant at a 1% significance level. In Column 5, I control for observable contract characteristics (such as loan amount, loan maturity, and interest rate) and borrower indebtedness. The estimate becomes only slightly smaller than in Table 5. Hence, there is an increase in *ex post* default risk, even after controlling for unobservable economic trends, observable loan characteristics, or higher borrower indebtedness at loan origination.

[Table 7 here]

3.3.2 Screening Effort

In this section, I exploit the cross-sectional variation in lead bank's incentives and information to obtain further insights about the underlying source of higher defaults after the regulation. The central concern is moral hazard, because loan sales weaken banks' monitoring incentives. In particular, I attempt to understand whether lead banks become less likely to collect soft information, or whether they originate riskier loans because they have less exposure to default. I hypothesize that defaults will be higher for loans in which lead banks retain very little stakes or those that require them to collect more information.

In Table 8 (Column 1), I interact the right-hand side $event_{jt}$ variable with the dummy variable $low\ lead\ share_{ijt}$ that indicates whether the lead bank j retains less than 5% stake in loan ijt. I find that speculative-grade loans where the lead bank retains less than 5% are 26.06 percentage more likely to experience borrower defaults compared to other speculative-grade deals under the new rules. This strengthens the evidence in Section 3.1 that there is a moral hazard problem because lead banks that retain a smaller portion of speculative-grade loans have weaker monitoring incentives.

[Table 8 here]

Lenders must exert an unobservable effort to collect soft information, and lower exposure to default reduces their effort incentives. If the regulation results in weaker screening effort, defaults should be higher for loans in which lead banks know very little about *ex ante* borrower quality. In Table 8 (Column 1), I interact the right-hand side $event_{jt}$ variable with the dummy $prior\ lending\ relation_{ijt}$ that indicates whether the lead bank j has a prior lending relation with borrower i before the regulation. I find that these loans are 5.38 percentage points higher among borrowers with prior lending relations to lead banks. This finding does not support the screening hypothesis, but instead suggests that lead banks are *ex ante* informed about the riskiness of the borrowers. In Column 2-4, I show that loans with lead banks with low lending expertise in the syndicated loan markets (Column 1), low industry specialization (Column 2), and low private information on borrowers (Column 3) are significantly less likely to exhibit defaults. Thus, I find no evidence that lead banks exert less screening effort but instead show that banks originate riskier loans because they have less exposure to default.

3.3.3 Adverse Selection vis-à-vis Shadow Banks

The banking literature argues that banks have a comparative information advantage relative to other lenders based on two ideas. First, banks build up experience and specialization in monitoring borrowers through time (Fama, 1985). Second, banks form repeated interactions with their borrowers and accumulate firm-specific and private information such as detailed knowledge of their borrowers' assets and investment opportunities (Boot, 2000). Due to less lending and monitoring experience, and shorter longevity of lending relationships, shadow banks are less likely to know about the true quality of the loans. Second, shadow banks likely lack the proper incentives to screen and monitor because of their little stakes. Thus, I predict that the rise in defaults should be greater among the loans distributed to shadow banks who have less ability and incentives to monitor credit quality.

To test this hypothesis, I modify Equation 3 in Table 9 by interacting the right-hand side variable $event_{ijt}$ with dummies that define a pecking order of non-bank participation where higher non-bank participation means that non-banks are more likely to distort monitoring. In Column 1, I interact $event_{ijt}$ with a dummy variable that takes the value of one if $loan_{ijt}$ has a non-bank lead arranger (*i.e.*, co-originated with non-bank institutional investors). I find that the relative increase in *ex post* borrower defaults after the risk-sensitive capital regulation is 7.13 percentage points higher for loans that have a non-bank lead arranger (monitor). This is consistent with the view that non-banks are less likely to monitor borrowers. In Column 2, I find that the wedge in default probability is 5.8 percentage points higher for loans with a non-bank participant at loan origination, and also sold to non-banks in the secondary loan markets. I also show that the increase in defaults is 5.74 percentage points higher for loans with a non-bank participant (Column 3), and 2.91 percentage points greater for loans that are sold to non-banks in the secondary loan markets (Column 4). In contrast, the relative increase in default probability is 5.55 percentage points less for contracts with only bank participation (Column 5). In Columns 6 to 10, I replace the dependent variable with $default_{ijt}$ and obtain similar results. The wedge is highest for loans with a non-bank monitor (Column 6), and negative for loans with only bank participation (Column 10). Taken together, these results are consistent with monitoring distortions, and reveal a pecking order of default risk where the latter is highest when non-banks have a monitoring role (*i.e.*, they are lead arrangers).

[Table 9 here]

The rise in default risk could be higher among loans distributed to shadow banks because the latter are less able to know the true quality of loans (information channel), but also because they lack the proper incentives to screen and monitor (incentive channel). If shadow banks free-ride on lead bank's information collection and they do not control credit quality, they are less likely to require the lead bank to monitor. I therefore exploit the cross-sectional variation in shadow banks' information and incentives to obtain further insights about the underlying source of lax screening. I predict the following. The rise in defaults after the regulation is higher among loans distributed to non-banks with low monitoring ability and/or incentives.

In Table 10, Column 1–4, I test the degree of asymmetric information among non-bank participants. In Column 1, I hypothesize that non-bank subsidiaries of banks likely benefit from the private information of their bank parents, and that they are more informed relative to non-bank lenders that have no bank affiliations[8]. To account for bank affiliation as a source of in information asymmetry, I modify Specification 3 by interacting the right-hand side $event_{jt}$ variable with a *no bank affiliation*$_{ijt}$ dummy that indicates whether loan ijt has non-bank participants that are not owned or controlled by banks (*i.e.*, with a parent or ultimate parent that is not classified as a commercial or investment bank by Dealscan). Consistent with the view that a bank parent should improve monitoring, I find that for loans with non-bank participants who are not affiliated with banks, the relative wedge in the probability of default after the regulation is 11.96 percentage points larger.

[Table 10 here]

In Column 2, I exploit non-bank experience in syndicated loan markets as a source of asymmetric information among non-bank participants. If non-banks have lending expertise, they should better assess borrower credit quality. To address this channel, I interact $event_{jt}$ with the dummy variable *low lending expertise*$_{ijt}$ that takes one if the average lending experience of non-banks at loan origination is less than five years. I find that the relative increase in defaults is 15.01 percentage points greater for loans with less experienced non-bank participants. In Column 3, I consider non-banks' industry specialization, as participants with expertise in the borrower's industry are likely to better assess the credit quality of their borrowers (Ivashina, 2009). To test this channel, I interact $event_{jt}$ with *low industry expertise*$_{ijt}$ that has the value of one if the average industry-specific lending experience of non-banks is less than five years. I show that the relative increase in defaults is 16.04 percentage points greater for loans with non-banks with low industry-specific expertise.

According to Petersen and Rajan (1994), Berger and Udell (1995), and Ivashina (2009), repeated lending reduces information asymmetry about the borrower. Thus, in Column 4, I interact $event_{jt}$ with the dummy variable *short relation with borrower*$_{ijt}$ that takes the value of one if the average length of lending relation with borrower i of non-bank participants is less than five years. Similarly, the wedge in default probability is 7.86 percentage points higher for the loans with less informed non-banks. Taken together, these results are consistent with the

[8] For example, a CLO has an underwriter (typically a bank) responsible for screening the loan portfolio, and rating and pricing the CLO tranches (Benmelech Dlugosz, and Ivashina, 2012). For this type of non-bank institutional investors, banks exert direct influence on screening.

hypothesis that information asymmetry is the key channel for which the increase in default probability is higher among the loans distributed to non-banks.

In Column 5, I test the incentive channel. To do so, I compare differences in *ex post* borrower defaults for the deals where non-banks on average keep less than 5% with other loans and I find no statistically significant difference. This suggests that the incentive channel is not borne out in the data. Thus, when a lead bank shrinks loan retention, non-banks with lesser expertise and information – and not fewer incentives – bear higher credit risk among loans originated by the same bank.

The syndicated loan market is a private market and the choice of participants is largely determined by the lead bank (Benmelech, Dlugosz, and Ivashina, 2009). Thus, there could be a tacit market segmentation that causes banks subject to the rules to strategically sell higher quality loans to other banks and to non-banks with established presence, and distribute riskier loans to lenders that are new entrants, lack industry specialization, or private information[9]. However, uninformed non-banks might internalize adverse selection by submitting bids that are insensitive to credit quality regardless of whether banks strategically target them to distribute lower-quality loans. Due to lack of data on the (pre-) syndication process, I cannot yet distinguish between these two explanations. Thus, the paper is agnostic about the exact mechanism through which lead banks distribute risky loans to uninformed shadow banks.

The interpretation of Table 10 is that uninformed non-banks internalize adverse selection more because they had less ability to check credit quality. However, one concern is that the latter might willingly accept risk in a search-for-yield. To test whether uninformed non-banks are compensated for internalizing higher credit risk, I augment Table 12 by adding the $allindrawn_{ijt}$, variable – which corresponds to the initial amount that borrower i pays in basis points over LIBOR for each dollar drawn down, including the fees. In Column 2, 4, 6, 8, 10, I find no evidence that uninformed non-banks who bear higher credit risk are compensated with a higher interest rate at loan origination[10]. This strengthens the evidence that non-banks bear higher credit risk because they have no adequate technology to check credit quality. Thus, their pricing schedule does not demonstrate sensitivity to the increase in credit risk.

[9]In general, litigation between syndicate members is rare because: (a) syndicate loans are not regulated by the Security Act of 1933 and (b) loan agreements typically limit the lead arranger's liability (Ivashina 2009).

[10]This finding is supportive of the link between non-bank institutional demand and credit mispricing in the leveraged loan markets (Ivashina and Sun, 2011).

[Table 11 here]

This analysis is inconclusive on the efficiency of trades from the perspective of uninformed shadow banks. It is possible that the latter sell their stakes at a premium in the secondary loan markets, and they do not make a loss. Answering this question requires the knowledge of trades and expected returns for each syndicate participant. However, no dataset that I am aware of contains this information. In the Appendix, I investigate potential explanations and I report evidence of learning on behalf of shadow banks. I show that banks with high historical defaults become less likely to distribute loans to shadow banks in the secondary loan markets (Table B2). This suggests that while risky loans are originated through uninformed shadow banks, adverse selection is broadly internalized in the secondary loan markets — reducing subsequent liquidity. While I do not find that shadow banks invest in risky loans for motives such as access to other businesses (Table B3), I cannot rule out the possibility that they willingly internalize adverse selection to gain market share in syndicated lending.

Finally, I investigate whether there is evidence of adverse selection vis-à-vis bank participants. Put differently, I examine whether loans extended to less informed bank participants exhibit more defaults after the regulation. The results are reported in Table 12. I find that for loans with bank participants with low lending expertise in syndicated lending (Column 1), low industry specialization (Column 2), low private information on borrowers (Column 3), and low incentives (Column 4), the difference in default probability is not statistically significant. This shows that the information and incentive channel shut down for banks. It also suggests that shadow banks bear higher credit risk because they have no adequate monitoring technology.

[Table 12 here]

3.4 Does default risk rise due to lax monitoring *ex post*?

In this section, I investigate whether defaults rise because banks monitor less *ex post*. To this end, I take two steps. First, I analyse whether loans originated under the risk-sensitive capital regulation are associated with laxer covenants. Second, I test whether borrowers engage in risky behavior after loan origination because banks monitor less.

3.4.1 Covenant Analysis

In this section, I study whether defaults in non-bank loans subject to the risk-sensitive capital regulation rise because banks monitor less *ex post*. This could happen because debt ownership becomes more dispersed and makes renegotiation more difficult. To test for this, I analyse the existence and strictness of loan covenants.

Prior literature argues that banks monitor borrowers by designing restrictive covenants (*e.g.*, Smith and Warner, 1979; and Rajan and Winton 1995). Covenants are used to protect, and signal the ability of borrowers to pay back their debt. They operate through two mechanisms: limit the control rights of the management (for example by preventing the firm from undertaking risky and opportunistic behaviour in terms of equity distributions, additional debt, or new acquisitions), and give banks the right to renegotiate the debt contracts *ex post* (for example by imposing default or stricter contractual terms or accelerating payments[11]) in the event of a violation. There is empirical evidence that covenants reduce the cost of debt capital *ex ante* (Reisel, 2014; and Bradley and Roberts, 2015); and create shareholder value *ex post* (Chava and Roberts, 2008; and Nini, Smith and Sufi, 2009, 2012). Thus, I test the following: If banks subject to risk-sensitive capital monitor less, they will impose looser covenants.

Detailed description of loan covenants in Dealscan allows me to exploit different measures of monitoring. The results are collected in Table 13. I find that banks subject to risk-sensitive capital experience a 16.73% decrease in the number of covenants in loans co-originated with non-banks (Column 1), and 28.82% drop in loans distributed to non-banks in the secondary loan markets (Column 2). To understand what type of covenants is removed, I further classify them into three groups (debt covenant, investment covenant, and value covenant). On the extensive margin, the regulation is only associated with a removal of debt covenants — (that limit the firm's indebtedness). Speculative-grade loans subject to higher capital charges and co-originated with non-banks are 22.12 percentage points less likely to have a debt covenant (Column 3), and speculative-grade contracts subject to higher capital charges and distributed to non-banks in the secondary loan markets are 18.43 percentage points less likely to have a debt covenant (Column 4). I observe no differences in the probability of having a value covenant — (that requires the firm to keep up with minimum profitability and net worth value requirements), or an investment covenant (Column 5-8). The probability of having a dividend

[11]See, for example, Roberts and Sufi, 2009.

restriction, however, goes down, with 5.69 percentage points drop for loans co-originated with non-banks (Column 9) and 21.04 percentage points reduction for non-bank term loans (Column 10).

[Table 13 here]

Next, I examine covenant strictness and enforcement. In terms of covenant strictness, I use the Murfin (2012) covenant strictness measure — which measures the probability that a borrower may violate its covenants, considering the correlation and interaction of various covenants in the contract. Using the Murfin measure, I show that after the risk-sensitive capital regulation, banks relax covenant strictness by 4.86% for speculative-grade loans co-originated with non-banks (Column 11), and by 5.45% for speculative-grade loans with a non-bank term tranche (Column 12). This estimate is close to that reported by Wang and Xia (2014), who have documented that covenant strictness decreases by 8.4% when a lender switches from non-securitization active to securitization active. Finally, I find that non-bank loans are substantially more likely to be cov-lite[12]; thus, they have weaker covenant enforcement. The probability of being cov-lite increases by 4.03 percentage points for loans with a non-bank lead (Column 13), and 36.69 percentage points for loans with a non-bank term tranche.

Hence, banks subject to risk-sensitive capital monitor less after loan origination: they relax covenants limiting borrowers' indebtedness. As shown in Table 6, the looser covenants in speculative-grade loans subject to risk-sensitive capital do not merely reflect *ex ante* lower credit risk. Instead, initial leverage ratios are higher.

3.4.2 Firm-Level Analysis

In this section, I investigate whether borrowers behave opportunistically and engage in risky behavior after loan origination because banks subject to the regulation monitor less. To understand changes in firm-level outcomes after loan origination, I run the following regression specification[13]:

[12] The definition taken from Ivashina and Sun (2017) states that a cov-lite (*i.e.*, a contract with incurrence provisions) requires the firm to comply with financial covenants only in the case of an active event — such as issuance of additional financing, sale of assets, or merger. For example, a cov-heavy contract requires the firm to comply with a leverage cap at all points in time, while a cov-lite contract is enforced when the firm raises additional financing.

[13] The sample is limited to firms which appear in Dealscan database at least once, and for which Compustat data are available.

$$y_{it} = \alpha_i + \alpha_t + \beta * After_{it} + \gamma * X_{it} + \epsilon_{it} \quad (4)$$

where y_{it} is replaced with firm-level outcomes. The dummy variable $After_{it}$ is one from the year when firm i with a speculative-grade credit rating takes out a loan from a bank that is subject to risk-sensitive capital regulation. The results are reported in Table 14. I find that while debt contracts originated by affected banks are followed by a rise in total debt (Column 1, Panel A); they are associated with no significant changes in firms' investment (Panel B, Column 1). Instead, I observe that after loan origination, firms improve their liquidity position, and increase their cash holdings by 17.52% (Column 2, Panel B). I observe no significant changes in net working capital (Column 3, Panel B), employment (Column 4, Panel B) and in the fraction of profits that firms pay to shareholders (Column 5, Panel B). Thus, the increase in firms' leverage leads to a rise in cash ratio, rather than an increase in capital expenditures.

[Table 14 here]

I further examine changes in firms' leverage and profitability. After borrowing from a bank affected by risk-sensitive capital, firms' debt to assets ratio increases by 6.93 percentage points (Column 1, Panel A); net debt to assets ratio increases by 7.44 percentage points (Column 2, Panel A); and long-term debt to assets ratio increases by 7.17 percentage points (Column 3, Panel A). Specifically, firms increase debt maturity after borrowing from affected banks: while there is no significant change in the amount of long-term debt due in the first year (Column 4, Panel A), the amount of long-term debt maturing in the second-year, third-year, fourth-year and fifth year rises by 19.76% (Column 5, Panel A), 35.19% (Column 6, Panel A), 53.57% (Column 6, Panel A), 59.93% (Column 7, Panel A), respectively. Moreover, after taking out a loan from an affected bank, firms' profitability decreases by 2.68 percentage points (Column 6, Panel B). I also observe 13.80% drop in market value (Column 7, Panel B) and 15.73% decrease in the stock price (Column 8, Panel B).

Hence, I find no evidence of moral hazard on behalf of the borrowers. This strengthens the evidence that the increase in default risk is due to lax screening *ex ante*, as opposed to lax monitoring *ex post*. In untabulated regressions, I confirm that the increase in *ex post* defaults under the new regulation are systematically and negatively linked to *ex ante* investment opportunities. This strengthens the evidence that firm-level outcomes are driven by *ex ante* firm quality, as opposed to *ex post* moral hazard on behalf of the borrowers.

4 Conclusion

In this paper, I study the risk implications of bank capital requirements in the presence of shadow banks. I show that in response to higher capital charges as an exogenous shock to loan retention, banks originate riskier loans, and transfer them to shadow banks. Under the risk-sensitive capital rules, speculative-grade borrowers default systematically more *ex post* – with this effect being confined to non-bank loans. Consistent with strategic adverse selection, defaults are higher for loans that are distributed to uninformed non-banks, and on which lead banks have more private information. Although banks relax covenants and monitor less *ex post*, I do not find evidence of *ex post* moral hazard on behalf of the borrowers. After loan origination, borrowers increase debt maturity and pile cash.

These results show that in the originate-to-distribute system, bank capital regulation distorts screening incentives. Prior empirical work has shown that banks in the traditional originate-to-hold model react to higher capital charges by lending less (Behn, Haselmann, and Wachtel 2015). I find that in the originate-to-distribute system, banks sell the loans that they originate, and that they, as a result, have weaker incentives to screen the riskiness of their borrowers. The riskier loans are absorbed by shadow banks. These results are consistent with recent models that argue that in the presence of shadow banks, bank capital requirements could backfire (see, *e.g.*, Plantin 2014). The amount of risk that is originated by banks, and distributed to shadow banks is larger. To the extent that shadow banks are interconnected with banks, the risk that is transferred to shadow banks might stay within the banking system.

While the purpose of banking regulations is to ensure the safety of financial system, they will backfire if they distort banks' monitoring. I therefore argue that bank regulations should not be studied in isolation from incentives. One potential regulatory response is to complement bank capital reforms with rules that align incentives within and across distinct players in the economy. For example, regulators could target the compensation structure in banks, and link bankers' long-term pay to the long-term health of their borrowers and their reputation vis-à-vis unsophisticated market participants. Another option is to impose minimum loan retention requirements on the banks to ensure adequate monitoring incentives. Finally, the results in this paper suggest a positive link between bank capital regulation and financial crises in the presence of regulatory arbitrage. This is an important area for future research.

Table 1: Countries affected by risk-sensitive capital

Event Year	Affected Countries
2004	Canada, Australia, Belarus, Brazil, Malaysia
2005	EU, New Zealand, India, Hong Kong, Colombia, Pakistan, Qatar, Kuwait, Gibraltar, Mongolia
2006	Costa Rica, Philippines, Morocco, Oman, Sri Lanka
2007	Japan, South Korea, Bahrain, Bangladesh, Singapore, Taiwan, Israel, Thailand, Saudi Arabia, Lebanon
2008	Armenia, Bermuda, Jordan, UAE, Mauritius, South Africa, Mexico, Croatia, Montenegro, Nepal
2009	China, Brunei, Russia, Serbia, Peru, Maldives
2010	Cayman Islands, Namibia
2011	Indonesia
2012	Egypt, Turkey, Macedonia, Uruguay
2013	Albania, Botswana, Georgia, Kosovo, Malawi, Mozambique, Vanuatu, Liberia
2014	Argentina, Nigeria

Table 2: Validation of the Empirical Setting

Extensive Margin	(1)	(2)	(3)	(4)
Dep. Var.:	$Lead\, arranger_{ijt}$		$Bank\, market\, share_{jt}$	
	Speculative	Non-speculative	Speculative	Non-speculative
$event_{jt}$	-0.1005***	-0.0133	-0.0133***	-0.0004
	(0.0229)	(0.0280)	(0.0013)	(0.0012)
Observations	34678	43648	5921	10511
R-squared	0.438	0.541	0.658	0.585
Year FE	No	No	Yes	Yes
Loan FE	Yes	Yes	No	No
Lender FE	No	No	Yes	Yes
Lender Parent Country FE	Yes	Yes	Yes	Yes
Lender State FE	Yes	Yes	Yes	Yes
Cluster	Loan	Loan	Lender	Lender

This table reports results concerning the extensive margin of bank lending. In Column 1 and 3, the sample is reduced to speculative-grade loans, and in Column 2 and 4, it is restricted to loans other than those with speculative-grade ratings. The dummy variable $event_{jt}$ is one in the post-regulation period after bank j becomes subject to risk-sensitive capital regulation. In Columns 1–2, the unit of observation is facility level, there is one observation per lender-facility pair, and the dependent variable *lead arranger*$_{ijt}$ is a dummy that equals one if lender receives lead arranger credit in the syndicate based on Reuters LPC's League Table guidelines. In Column 1, I test whether bank j becomes less likely to take lead arranger status within a syndicate after it becomes subject to the regulation. In Column 2, test whether bank j becomes less likely to take lead arranger status in a non-speculative-grade loan syndicate after the regulation. Controls include the state of the lender j, and the parent country of the lender j. Standard errors correct for clustering at the package-level, and are reported in parentheses. In Column 3-6, the unit of observation is collapsed to monthly-bank level. The dependent variable *bank market share*$_{jt}$ in Column 3 is the ratio of the number of speculative-grade loans originated by bank j at time t divided by the total number of speculative-grade loans originated at time t. In Column 4, the dependent variable *bank market share*$_{jt}$ is the ratio of the number of non-speculative-grade loans originated by bank j at time t divided by the total number of non-speculative-grade loans originated at time t. Controls include the state and parent country of lender j. Standard errors correct for clustering at the lender-level, and are reported in parentheses. The bottom of the table provides information about fixed-effects, and the level of clustering.***, ** and * indicate statistical difference from zero at the 1%, 5% and 10% levels, respectively. Sources: Dealscan (1990-2017).

Table 3: Risk-sensitive capital and originate-to-distribute

Intensive Margin Dep. Var.:	(1) $lead\,share_{ijt}$	(2) $log(\#\,total\,lenders)_{ijt}$	(3) *non-bank lead*$_{ijt}$
$event_{jt}$	-7.0416** (3.5114)	0.2421*** (0.0325)	0.1136*** (0.0247)
Observations	2440	6231	6231
R-squared	0.491	0.773	0.384
Year FE	Yes	Yes	Yes
Lender FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Loan Purpose FE	Yes	Yes	Yes
Lender Parent Country FE	Yes	Yes	Yes
Borrower State FE	Yes	Yes	Yes
Lender State FE	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes
Cluster	Lender	Lender	Lender

This table reports results concerning the intensive margin of bank lending using Equation 1 and 2. The sample is reduced to speculative-grade loans. The dummy variable $event_{jt}$ is one in the post-regulation period after bank j becomes subject to risk-sensitive capital regulation. In Column 1, the unit of observation is at facility-level, there is one observation per lead arranger-facility pair, and the dependent variable *lead share*$_{ijt}$ corresponds to the amount that lead arranger commits to a facility. In Column 2–3, the unit of observation is at the package-level, and there is one observation per lead arranger-package pair. In Column 2, the dependent variable *log(total lenders)*$_{ijt}$ is the natural logarithm of the total number of participants at loan origination. In Column 3, the variable *non-bank lead*$_{ijt}$ takes the value of one if the loan ijt has a non-bank lead arranger. Controls include the log of firm i's sales (defined by Dealscan as sales at close), and dummies for the type of loan, state of firm i, industry of firm i, state of lender j, and parent country of the lender j. The bottom of the table provides information about fixed-effects, and the level of clustering. Standard errors correct for clustering at the lender-level, and are reported in parentheses. ***, ** and * indicate statistical difference from zero at the 1%, 5% and 10% levels, respectively. Sources: Dealscan (1990-2017).

Table 4: Risk-sensitive capital, loan maturity and interest rate

Intensive Margin Dep. Var.:	(1) $log(maturity)_{ijt}$	(2) $log(bank\ maturity)_{ijt}$	(3) $log(non\text{-}bank\ maturity)_{ijt}$	(4) $allindrawn_{ijt}$
$event_{jt}$	-0.1034***	-0.1018***	-0.0806	5.0367
	(0.0345)	(0.0343)	(0.0579)	(8.5479)
Observations	6175	6079	3268	6124
R-squared	0.395	0.386	0.451	0.453
Year FE	Yes	Yes	Yes	Yes
Lender FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes
Lender Parent Country FE	Yes	Yes	Yes	Yes
Borrower State FE	Yes	Yes	Yes	Yes
Lender State FE	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Cluster	Lender	Lender	Lender	Lender

This table reports results concerning bank lending from Equation 1 and 2. The sample is reduced to speculative-grade loans. The dummy variable $event_{jt}$ is one in the post-regulation period after bank j becomes subject to the risk-sensitive capital regulation. In Column 1–3, the unit of observation is package-level, and there is one observation per lead arranger-package pair. In Column 1, the variable *log(maturity)*$_{ijt}$ is the natural logarithm of the maximum maturity of a facility in a package (*i.e.*, how long in months a facility will be active from signing to expiration date). The variable *log(bank maturity)*$_{ijt}$ in Column 2 is the natural logarithm of the maximum maturity of a bank tranche (*i.e.*, facility including bank participants), and the variable *log(non-bank maturity)*$_{ijt}$ in Column 3 is the natural logarithm of the maximum maturity of a non-bank tranche (*i.e.*, facility including non-bank participants) in a package. In Column 4, the unit of observation is at the facility-level; there is one observation per lead arranger-facility pair, and the dependent variable $allindrawn_{ijt}$ corresponds to the amount that borrower i pays in basis points over LIBOR for each dollar drawn down, including the fees. Controls include the log of firm i's sales (defined by Dealscan as sales at close), and dummies for the type of loan, state of firm i, industry of firm i, state of lender j, and parent country of lender j. The bottom of the table provides information about fixed-effects, and the level of clustering. Standard errors correct for clustering at the lender-level, and are reported in parentheses. ***, ** and * indicate statistical difference from zero at the 1%, 5% and 10% levels, respectively. Sources: Dealscan (1990-2017).

Table 5: Default Risk

	(1)	(2)	(3)	(4)	(5)	(6)
	Speculative-grade		Non-speculative-grade		All loans	
Dep. Var.:	$default_{ijt}$	$bankrupt_{ijt}$	$default_{ijt}$	$bankrupt_{ijt}$	$default_{ijt}$	$bankrupt_{ijt}$
$event_{jt}$	0.0828***	0.0724***	-0.0604*	-0.0558*	0.0220	0.0163
	(0.0276)	(0.0256)	(0.0340)	(0.0294)	(0.0233)	(0.0170)
Observations	4575	4575	7005	7005	11580	11580
R-squared	0.245	0.256	0.222	0.226	0.172	0.180
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Lead Arranger FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes	Yes	Yes
Lender Parent Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Borrower State FE	Yes	Yes	Yes	Yes	Yes	Yes
Lender State FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Lender	Lender	Lender	Lender	Lender	Lender

This table reports the results concerning default risk using Equation 3. In Columns 1 to 2, the sample is reduced to speculative-grade loans, and in Columns 3 to 4, it is restricted to non-speculative-grade loans. Columns 5-6 include all Dealscan loans. In Columns 1, 3, and 4, the dependent variable $default_{ijt}$ takes the value of one if firm i defaults any time within five years of taking out the loan. Default is defined as any event classified as default by the Moody's Default and Recovery Database (DRD). In Columns 2, 4, and 6, the dependent variable $bankrupt_{ijt}$ takes the value of one if the borrower goes bankrupt any time within five years of taking out a loan. Bankruptcy is any event classified as bankruptcy by the Moody's Default and Recovery Database (DRD). The dummy variable $event_{jt}$ is one in the post-regulation period after bank j becomes subject to the risk-sensitive capital regulation. Controls include the log of firm i's sales (defined by Dealscan as sales at close), and dummies for the type of loan, state of firm i, industry of firm i, state of lender j, and parent country of lender j. The bottom of the table provides information about fixed-effects, and the level of clustering. Standard errors correct for clustering at the lender-level, and are reported in parentheses. ***, ** and * indicate statistical difference from zero at the 1%, 5% and 10% levels, respectively. Sources: Dealscan and DRD (1990-2013).

Table 6: *Ex ante* Borrower Indebtedness

Dep. Var.:	$debt/assets_{it-1}$
$event_{jt}$	0.0437**
	(0.0207)
Observations	3504
R-squared	0.516
Industry-Year FE	Yes
State-Year FE	Yes
Firm Controls	Yes
Loan Purpose FE	Yes
Lender Parent Country FE	Yes
Lender State FE	Yes
Lead Arranger FE	Yes
Cluster	Lender

This table reports the results concerning changes in *ex ante* borrower riskiness. I replace the dependent variable in Equation 3 with $debt/assets_{it-1}$. The dummy variable $event_{jt}$ is one after bank j becomes subject to risk-sensitive capital regulation. The variable $debt/assets_{it-1}$ is the debt to assets ratio of borrower i one year prior to loan origination. Controls include the log of firm i's sales (defined by Dealscan as sales at close), and dummies for the type of loan, state of firm i, industry of firm i, state of lender j, and parent country of lender j. The bottom of the table provides information about fixed-effects, and the level of clustering. Standard errors correct for clustering at the lender-level, and are reported in parentheses. ***, ** and * indicate statistical difference from zero at the 1%, 5% and 10% levels, respectively. Sources: Dealscan and Compustat (1990-2017).

Table 7: Default Risk with Trends and Observables

	(1)	(2)	(3)	(4)	(5)
Dep. Var.:			$default_{ijt}$		
$event_{jt}$	0.0799***	0.0936***	0.0828***	0.0914***	0.0800***
	(0.0273)	(0.0287)	(0.0276)	(0.0293)	(0.0276)
$log(amount_{ijt})$					0.0132*
					(0.0076)
$log(maturity_{ijt})$					-0.0542**
					(0.0242)
$allindrawn_{ijt}$					0.0003***
					(0.0001)
$debt/assets_{it-1}$					0.1926***
					(0.0286)
Observations	4575	4575	4575	4575	3409
R-squared	0.441	0.293	0.245	0.475	0.292
Year FE	Yes	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	No	No	Yes	No
Industry-Year FE	No	Yes	No	Yes	No
Sales-Year FE	No	No	Yes	Yes	No
Industry FE	Yes	No	Yes	No	Yes
Borrower State FE	No	Yes	Yes	No	Yes
Lead Arranger FE	Yes	Yes	Yes	Yes	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes	Yes
Lender Parent Country FE	Yes	Yes	Yes	Yes	Yes
Lender State FE	Yes	Yes	Yes	Yes	Yes
Cluster	Lender	Lender	Lender	Lender	Lender

This table reports the results concerning default risk using Equation 3 and controlling for trends and observables at loan origination. The dependent variable $default_{ijt}$ is one if firm i defaults any time within five years of taking out the loan. The dummy variable $event_{jt}$ is one after bank j becomes subject to the risk-sensitive capital regulation. Controls include the log of firm i's sales (defined by Dealscan as sales at close), and dummies for the type of loan, state of firm i, industry of firm i, state of lender j, and the parent country of lender j. $log(amount_{ijt})$ is the total size of the loan, $log(maturity_{ijt})$ is the maximum maturity on the loan, $allindrawn_{ijt}$ is the rate on the loan, and $debt/assets_{it-1}$ is the indebtedness of the borrower one year before loan origination. The bottom of the table provides information about fixed-effects, and the level of clustering. Standard errors correct for clustering at the lender-level, and are reported in parentheses. ***, ** and * indicate statistical difference from zero at the 1%, 5% and 10% levels, respectively. Sources: Dealscan, Compustat and DRD (1990-2013).

Table 8: Heterogeneity by Lead Arranger Monitoring Expertise and Incentives

	(1) Monitoring Incentives	(2) Lending Relation	(3) Lending Expertise	(4) Industry Expertise	(5) Relation Length
Dep. Var.:			$default_{ijt}$		
$event_{jt}$	0.0797***	0.0794***	0.0923***	0.0888***	0.1403***
	(0.0280)	(0.0274)	(0.0288)	(0.0281)	(0.0417)
$event_{jt}$*$low\,lead\,share_{jt}$	0.2606***				
	(0.0539)				
$event_{jt}$*$prior\,lending\,relation_{jt}$		0.0538**			
		(0.0252)			
$event_{jt}$*$low\,lending\,expertise_{jt}$			-0.1905***		
			(0.0593)		
$event_{jt}$*$low\,industry\,expertise_{jt}$				-0.1233***	
				(0.0433)	
$event_{jt}$*$short\,relation\,with\,borrower_{jt}$					-0.0694**
					(0.0316)
Observations	4575	4575	4575	4575	4575
R-squared	0.246	0.248	0.247	0.247	0.246
Year FE	Yes	Yes	Yes	Yes	Yes
Lender FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes	Yes
Lender Parent Country FE	Yes	Yes	Yes	Yes	Yes
Borrower State FE	Yes	Yes	Yes	Yes	Yes
Lender State FE	Yes	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes
Cluster	Lender	Lender	Lender	Lender	Lender

This table reports the results concerning default risk using Equation 3 for loans with different levels of lead bank expertise and incentives at loan origination. The dependent variable $default_{ijt}$ is one if firm i defaults any time within five years of taking out the loan. The dummy variable $event_{jt}$ is one after bank j becomes subject to risk-sensitive capital regulation. The variable *low lead share*$_{ijt}$ takes the value of one if lead arranger j has less than 5% participation in loan ijt at loan origination and zero otherwise. The variable *prior lending relation*$_{ijt}$ is one if lead arranger j originated a loan for borrower i before the event. The variable *low lending expertise*$_{jt}$ takes the value of one if lead arranger j has less than five years of lending expertise at time t (i.e., the number of years spent at time t after arranging its first loan). The variable *low industry expertise*$_{ijt}$ has the value of one if lead arranger j has less than five years of industry-specific lending expertise at time t (*i.e.*, the number of years spent at time t after arranging the first loan in the industry of borrower i). The variable *short relation with borrower*$_{ijt}$ is one if lead arranger j has less than five years of lending relation with borrower i at time t (*i.e.*, the number of years spent at time t after arranging the first loan for borrower i). Controls include the log of firm i's sales (defined by Dealscan as sales at close), and dummies for the type of loan, state of firm i, industry of firm i, state of the lender j, and parent country of lender j. The bottom of the table provides information about fixed-effects, and the level of clustering. Standard errors correct for clustering at the lender-level, and are reported in parentheses. ***, ** and * indicate statistical difference from zero at the 1%, 5% and 10% levels, respectively. Sources: Dealscan and DRD (1990-2013).

Table 9: Pecking Order of Default Risk Migration

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep. Var.:			$default_{ijt}$					$bankrupt_{ijt}$		
$event_{ijt}$	0.0672**	0.0582**	0.0473*	0.0638*	0.0912***	0.0588**	0.0523*	0.0469*	0.0445	0.0824***
	(0.0284)	(0.0291)	(0.0274)	(0.0332)	(0.0273)	(0.0255)	(0.0271)	(0.0272)	(0.0284)	(0.0254)
$event_{ijt}$*non-bank $lead_{ijt}$	0.0713***					0.0623***				
	(0.0214)					(0.0192)				
$event_{ijt}$*non-bank participant and non-bank term $loan_{ijt}$		0.0580***					0.0473***			
		(0.0150)					(0.0158)			
$event_{ijt}$*non-bank $participant_{ijt}$			0.0574***					0.0411***		
			(0.0123)					(0.0137)		
$event_{ijt}$*non-bank term $loan_{ijt}$				0.0291*					0.0427***	
				(0.0164)					(0.0126)	
$event_{ijt}$*bank-only $loan_{ijt}$					-0.0555***					-0.0659***
					(0.0177)					(0.0169)
Observations	4575	4575	4575	4575	4575	4575	4575	4575	4575	4575
R-squared	0.247	0.247	0.247	0.246	0.246	0.258	0.257	0.257	0.257	0.257
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lender FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lender Parent Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Borrower State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lender State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Lender	Lender	Lender	Lender	Lender	Lender	Lender	Lender	Lender	Lender

This table reports the results concerning default risk using Equation 3 for loans with different levels of non-bank involvement. In Columns 1 to 5, the dependent variable $default_{ijt}$ takes the value of one if firm i defaults any time within five years of taking out the loan. In Columns 6 to 10, the dependent variable $bankrupt_{ijt}$ is one if the borrower goes bankrupt any time within five years of taking out a loan. The dummy variable $event_{ijt}$ is one after bank j becomes subject to the risk-sensitive capital regulation. The variable *non-bank* $lead_{ijt}$ takes the value of one if the loan contract ijt has a non-bank lead arranger in the syndicate at loan origination. The variable *non-bank participant and non-bank term* $loan_{ijt}$ is one if loan ijt has a non-bank participant in the syndicate at loan origination, and also has a non-bank term tranche (defined by Dealscan as Term Loan or Term Loan with higher-order designations than A.) The variable *non-bank* $participant_{ijt}$ has the value of one if loan ijt has a non-bank participant in the syndicate at loan origination. The variable *non-bank term* $loan_{ijt}$ is one if loan ijt has a non-bank term tranche. The variable *bank-only* $loan_{ijt}$ takes on the value of one if loan ijt neither has non-bank lead arrangers or participants, nor a non-bank term tranche. Controls include the log of firm i's sales (defined by Dealscan as sales at close), and dummies for the type of loan, state of firm i, industry of firm i, state of lender j, and parent country of lender j. The bottom of the table provides information about fixed-effects, and the level of clustering. Standard errors correct for clustering at the lender-level, and are reported in parentheses. ***, ** and * indicate statistical difference from zero at the 1%, 5% and 10% levels, respectively. Sources: Dealscan and DRD (1990-2013).

Table 10: Heterogeneity by Non-Bank Participant Monitoring Expertise and Incentives

	(1) Bank Affiliation	(2) Lending Expertise	(3) Industry Expertise	(4) Relation Length	(5) Monitoring Incentives
Dep. Var.:			$default_{ijt}$		
$event_{jt}$	0.0344	0.0320	0.0299	0.0361	0.0314
	(0.0347)	(0.0341)	(0.0340)	(0.0343)	(0.0341)
$event_{jt}$*non-bank $loan_{ijt}$	0.0334*	0.0510***	0.0475***	0.0373**	0.0557***
	(0.0193)	(0.0176)	(0.0175)	(0.0168)	(0.0189)
$event_{jt}$*non-bank $loan_{ijt}$*no bank $affiliation_{ijt}$	0.1196***				
	(0.0445)				
$event_{jt}$*non-bank $loan_{ijt}$*low lending $expertise_{ijt}$		0.1501*			
		(0.0890)			
$event_{jt}$*non-bank $loan_{ijt}$*low industry $expertise_{ijt}$			0.1604**		
			(0.0623)		
$event_{jt}$*non-bank $loan_{ijt}$*short relation with $borrower_{ijt}$				0.0786**	
				(0.0319)	
$event_{jt}$*non-bank $loan_{ijt}$*low loan $share_{ijt}$					0.0411
					(0.0312)
Observations	4575	4575	4575	4575	4575
R-squared	0.249	0.247	0.248	0.258	0.247
Year FE	Yes	Yes	Yes	Yes	Yes
Lender FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes	Yes
Lender Parent Country FE	Yes	Yes	Yes	Yes	Yes
Borrower State FE	Yes	Yes	Yes	Yes	Yes
Lender State FE	Yes	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes
Cluster	Lender	Lender	Lender	Lender	Lender

This table reports the results concerning default risk using Equation 3 for loans with different levels of non-bank participant monitoring expertise and incentives at loan origination. The dependent variable $default_{ijt}$ takes the value of one if firm i defaults any time within five years of taking out the loan. The dummy variable $event_{jt}$ is one after bank j becomes subject to risk-sensitive capital regulation. The variable *non-bank* $loan_{ijt}$ takes the value of one if loan ijt is either originated by a syndicate including non-banks or it has a non-bank term tranche. The variable *no bank* $affiliation_{ijt}$ is one if loan ijt has a non-bank participant that is not affiliated with banks. The variable *low lending* $expertise_{ijt}$ has the value of one if the average lending expertise of non-bank participants in the syndicate at loan origination is less than five years. The variable *low industry* $expertise_{ijt}$ has the value of one if the average industry-specific lending expertise of non-bank participants in the syndicate is less than five years. The variable *short relation with* $borrower_{ijt}$ is one if the average length of lending relation with borrower i of non-bank participants in the syndicate is less than five years. The variable *low loan* $share_{ijt}$ takes one if the average loan share of non-banks in the syndicate at loan origination is less than 5% and zero otherwise. Controls include the log of firm i's sales (defined by Dealscan as sales at close), and dummies for the type of loan, state of firm i, industry of firm i, state of lender j, and parent country of lender j. The bottom of the table provides information about fixed-effects, and the level of clustering. Standard errors correct for clustering at the lender-level, and are reported in parentheses. ***, ** and * indicate statistical difference from zero at the 1%, 5% and 10% levels, respectively. Sources: Dealscan and DRD (1990-2013).

Table 11: Heterogeneity by Non-Bank Participant: Default and Interest Rate

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Bank Affiliation		Lending Expertise		Industry Expertise		Relation Length		Monitoring Incentives	
Dep. Var.:	$default_{ijt}$	$allindrawn_{ijt}$	$default_{ijt}$	$allindrawn_{ijt}$	$default_{ijt}$	$allindrawn_{ijt}$	$default_{ijt}$	$allindrawn_{ijt}$	$default_{ijt}$	$allindrawn_{ijt}$
$event_{jt}$	0.0344	-75.1435***	0.0320	-79.7005***	0.0299	-78.8746***	0.0361	-76.0531***	0.0314	-84.2493***
	(0.0347)	(9.0480)	(0.0341)	(9.0959)	(0.0340)	(8.9872)	(0.0343)	(9.1281)	(0.0341)	(8.7863)
$event_{jt}$* *non-bank loan*$_{ijt}$	0.0334*	95.0433***	0.0510***	92.1925***	0.0475***	93.2216***	0.0373**	93.5749***	0.0557***	103.9576***
	(0.0193)	(6.0397)	(0.0176)	(5.9724)	(0.0175)	(6.0532)	(0.0168)	(6.9272)	(0.0189)	(6.6208)
$event_{jt}$* *non-bank loan*$_{ijt}$**no bank affiliation*$_{ijt}$	0.1196***	-30.4507***								
	(0.0445)	(11.2003)								
$event_{jt}$**non-bank loan*$_{ijt}$**low lending expertise*$_{ijt}$			0.1501*	17.6197						
			(0.0890)	(19.5388)						
$event_{jt}$**non-bank loan*$_{ijt}$**low industry expertise*$_{ijt}$					0.1604**	-14.2063				
					(0.0623)	(11.1747)				
$event_{jt}$**non-bank loan*$_{ijt}$**short relation with borrower*$_{ijt}$							0.0786**	-38.3794***		
							(0.0319)	(14.5263)		
$event_{jt}$**non-bank loan*$_{ijt}$**low loan share*$_{ijt}$									0.0411	-44.5739***
									(0.0312)	(11.8749)
Observations	4575	8247	4575	8247	4575	8247	4575	8247	4575	8247
R-squared	0.249	0.376	0.247	0.372	0.248	0.371	0.258	0.373	0.247	0.377
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lender FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lender Parent Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Borrower State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lender State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Lender	Lender	Lender	Lender	Lender	Lender	Lender	Lender	Lender	Lender

This table reports the results concerning default risk and interest rates for loans with different levels of non-bank participant monitoring expertise and incentives. In Columns 1, 3, 5, 7, and 9, the dependent variable $default_{ijt}$ is one if firm i defaults within five years of taking out the loan. In Columns 2, 4, 6, 8, and 10, the unit of observation is facility-level, there is one observation per lead arranger-facility pair, and the dependent variable $allindrawn_{ijt}$ corresponds to the amount that borrower i pays in basis points over LIBOR for each dollar drawn down, including the fees. The dummy variable $event_{jt}$ is one after bank j becomes subject to risk-sensitive capital regulation. The variable *non-bank loan*$_{jt}$ takes the value of one if loan ijt is either originated by a syndicate including non-bank participants or it has a non-bank term tranche. The variable *no bank affiliation*$_{jt}$ takes the value of one if loan ijt has a non-bank participant that is not affiliated with banks. The variable *low lending expertise*$_{jt}$ takes the value of one if the average lending expertise of non-bank participants at loan origination is less than five years. The variable *low industry expertise*$_{jt}$ has the value of one if the average industry-specific lending expertise of non-bank participants in the syndicate is less than five years. The variable *short relation with borrower*$_{ijt}$ is one if the average length of lending relation with borrower i of non-bank participants at loan origination is less than five years. The variable *low loan share*$_{ijt}$ takes the value of one if the average loan share of non-bank participants at loan origination is less than 5% and zero otherwise. Controls include the log of firm i's sales (defined by Dealscan as sales at close), and dummies for the type of loan, state of firm i, industry of firm i, state of the lender j, and parent country of lender j. The bottom of the table provides information about fixed-effects, and the level of clustering. Standard errors correct for clustering at the lender-level, and are reported in parentheses. ***, ** and * indicate statistical difference from zero at the 1%, 5% and 10% levels, respectively. Sources: Dealscan and DRD (1990-2013).

Table 12: Heterogeneity by Bank Participant Monitoring Expertise and Incentives

	(1) Lending Expertise	(2) Industry Expertise	(3) Relation Length	(4) Monitoring Incentives
Dep. Var.:	$default_{ijt}$			
$event_{jt}$	0.0879***	0.0843***	0.1163***	0.0826***
	(0.0291)	(0.0281)	(0.0394)	(0.0278)
$event_{jt}$*$low\ lending\ expertise_{ijt}$	-0.0807			
	(0.0697)			
$event_{jt}$*$low\ industry\ expertise_{ijt}$		-0.0830		
		(0.0615)		
$event_{jt}$*$short\ relation\ with\ borrower_{ijt}$			-0.0417	
			(0.0303)	
$event_{jt}$*$low\ loan\ share_{ijt}$				0.0445
				(0.0684)
Observations	4575	4575	4575	4575
R-squared	0.246	0.246	0.246	0.246
Year FE	Yes	Yes	Yes	Yes
Lender FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes
Lender Parent Country FE	Yes	Yes	Yes	Yes
Borrower State FE	Yes	Yes	Yes	Yes
Lender State FE	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Cluster	Lender	Lender	Lender	Lender

This table reports the results concerning default risk using Equation 3 for loans with different levels of bank participant monitoring expertise and incentives. The dependent variable $default_{ijt}$ is one if firm i defaults within five years of taking out the loan. The dummy variable $event_{jt}$ is one after bank j becomes subject to risk-sensitive capital regulation. The variable $low\ lending\ expertise_{ijt}$ takes the value of one if the average lending expertise of bank participants at loan origination is less than five years (*i.e.*, the average of number of years spent by each bank participant at time t after their first loan). The variable $low\ industry\ expertise_{ijt}$ has the value of one if the average industry-specific lending expertise of bank participants at loan origination is less than five years (*i.e.*, the average number of years spent by each bank participant at time t after their first loan in the industry of borrower i). The variable $short\ relation\ with\ borrower_{ijt}$ is one if the average length of lending relation with borrower i of bank participants at loan origination is less than five years (*i.e.*, the average number of years spent by each bank participant at time t after their first loan to borrower i). The variable $low\ loan\ share_{ijt}$ takes the value of one if the average loan share of bank participants at loan origination is less than 5% and zero otherwise. Controls include the log of firm i's sales (defined by Dealscan as sales at close), and dummies for the type of loan, state of firm i, industry of firm i, state of lender j, and parent country of lender j. The bottom of the table provides information about fixed-effects, and the level of clustering. Standard errors correct for clustering at the lender-level, and are reported in parentheses. ***, ** and * indicate statistical difference from zero at the 1%, 5% and 10% levels, respectively. Sources: Dealscan and DRD (1990-2013).

Table 13: Covenant Analysis

Dep. Var.:	(1) $log(\#covenants)_{ijt}$	(2)	(3) $debt\ covenant_{ijt}$	(4)	(5) $investment\ covenant_{ijt}$	(6)	(7) $value\ covenant_{ijt}$	(8)	(9) $dividend\ cap_{ijt}$	(10)	(11) $strictness_{ijt}$	(12)	(13) $cov\text{-}lite_{ijt}$	(14)
$event_{ijt}$	0.0078	0.1629***	0.0422	0.1157**	-0.049	-0.0503	0.0752***	0.0745***	-0.0378	0.0902*	-1.7029	1.1197	0.0785***	-0.1579***
	(0.0575)	(0.0549)	(0.0471)	(0.0478)	(0.0484)	(0.0502)	(0.0239)	(0.0256)	(0.0505)	(0.0494)	(1.6262)	(1.7346)	(0.0174)	(0.0245)
$event_{ijt}$*$non\text{-}bank\ lead_{ijt}$	-0.1673***		-0.2212***		0.0033		0.0189*		-0.0569**		-4.8607***		0.0403*	
	(0.0192)		(0.0184)		(0.0116)		(0.0096)		(0.0223)		(0.7423)		(0.0207)	
$event_{ijt}$*$non\text{-}bank\ term\ loan_{ijt}$		-0.2882***		-0.1843***		0.0029		0.0074		-0.2104***		-5.4463***		0.3669***
		(0.0237)		(0.0243)		(0.0172)		(0.0138)		(0.0161)		(0.8056)		(0.0189)
Observations	6595	6595	6595	6595	6595	6595	6595	6595	6595	6595	3843	3843	6595	6595
R-squared	0.274	0.271	0.301	0.301	0.389	0.389	0.302	0.312	0.369	0.378	0.242	0.245	0.364	0.439
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lender FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lender Parent Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Borrower State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lender State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Lender	Lender	Lender	Lender	Lender	Lender	Lender	Lender	Lender	Lender	Lender	Lender	Lender	Lender

This table reports the results concerning covenants from Equation 3 using loan-level data for loans with different types of non-bank involvement at loan origination. The dummy variable $event_{ijt}$ is one after bank j becomes subject to risk-sensitive capital regulation. In Columns 1 to 2, the dependent variable $log(\#covenants)_{ijt}$ is the log of the total number of different types of covenants included in loan package ijt. In Columns 3 and 4, the dependent variable $debt\ covenant_{ijt}$ takes the value of one if loan ijt has a financial covenant identified by Dealscan as "Max. Debt to EBITDA", "Max. Senior Debt to EBITDA", "Max. Debt to Tangible Net Worth", "Max. Leverage ratio", "Max. Debt to Equity", "Max. Senior Leverage", "Max. Loan to Value", "Max. Total Debt (including Contingent Liabilities) to Tangible Net Worth", and "Max. Net Debt to Assets". In Columns 5 to 6, the dependent variable $investment\ covenant_{ijt}$ is one if loan ijt has a financial covenant categorized by Dealscan as "Max. Capex" and "Max. Long-Term Investment to Net Worth", and in Columns 7 and 8, the dependent variable $value\ covenant_{ijt}$ has the value of one if loan ijt has a net worth covenant or a financial covenant classified by Dealscan as "Min. EBITDA". The dependent variable $dividend\ cap_{ijt}$ in Columns 9 and 10 takes the value of one if loan ijt has dividend restrictions at deal origination, and in Columns 11 and 12, the dependent variable $strictness_{ijt}$ is the Murfin covenant strictness measure. In Columns 13 to 14, the dependent variable $cov\text{-}lite_{ijt}$ is one if deal package ijt has a facility identified by Dealscan as "Cov-lite". The variable $non\text{-}bank\ lead_{ijt}$ takes the value of one if the loan contract ijt has a non-bank lead arranger in the syndicate at loan origination, and the variable $non\text{-}bank\ term\ loan_{ijt}$ is one if loan ijt has a non-bank term tranche. Controls include the log of firm i's sales (defined by Dealscan as sales at close), and dummies for the type of loan, state of firm i, industry of firm i, state of lender j, and parent country of lender j. The bottom of the table provides information about fixed-effects, and the level of clustering. Standard errors correct for clustering at the lender-level, and are reported in parentheses. ***, ** and * indicate statistical difference from zero at the 1%, 5% and 10% levels, respectively. Sources: Dealscan (1990-2017).

Table 14: Firm-Level Analysis

Panel A: Capital Structure	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Var.:	$(\frac{Debt}{Assets})_{it}$	$(\frac{Net\,Debt}{Assets})_{it}$	$(\frac{Long-term\,Debt}{Assets})_{it}$	$Log(dd1)_{it}$	$Log(dd2)_{it}$	$Log(dd3)_{it}$	$Log(dd4)_{it}$	$Log(dd5)_{it}$
$After_{it}$	0.0693***	0.0744***	0.0717***	0.0907	0.1976**	0.3519***	0.5357***	0.5993***
	(0.0147)	(0.0171)	(0.0134)	(0.0772)	(0.0805)	(0.0833)	(0.0821)	(0.0893)
Observations	79311	79307	141485	133828	109210	108671	108904	107025
R-squared	0.669	0.678	0.617	0.725	0.693	0.667	0.642	0.627
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm

Panel B: Real Effects	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Var.:	$(\frac{Investment}{Assets})_{it}$	$Log(Cash)_{it}$	$Log(WC)_{it}$	$Log(Employment)_{it}$	$(\frac{Payouts}{Assets})_{it}$	$(\frac{EBIT}{Assets})_{it}$	$Log(Market\,Value)_{it}$	$Log(Stock\,Price)_{it}$
$After_{it}$	0.0036	0.1752***	0.0452	0.0339	-0.0009	-0.0268***	-0.1380**	-0.1573***
	(0.0034)	(0.0626)	(0.0485)	(0.0314)	(0.0010)	(0.0065)	(0.0646)	(0.0503)
Observations	131866	134894	99273	129537	140233	133885	118602	125562
R-squared	0.586	0.871	0.883	0.957	0.423	0.596	0.918	0.756
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm

This table reports changes in firm-level outcomes after taking out a loan from a bank subject to risk-sensitive capital regulation. The sample is limited to firms which appear in Dealscan at least once during the sample period, and for which Compustat data are available. The dummy variable $After_{it}$ is one from the year when firm i with a speculative-grade credit rating takes out a loan from a bank that is subject to risk-sensitive capital regulation. In Panel A, the dependent variable is firm i's total debt to assets ratio in Column 1 and total long term-debt to assets ratio in Column 2. In Column 3-7, the dependent variable is the log of firm i's dollar amount of long-term debt that matures in the first, second, third, fourth, and fifth years from the balance sheet date. In Column 8, the dependent variable is the log of firm i's total debt minus cash to assets ratio. In Panel B, the dependent variable is the log of firm i's dollar amount of cash in Column 1, and it is investment to assets ratio in Column 2. In Column 3, the dependent variable is the log of firm i's number of employees, and in Column 4, it is the log of firm i's working capital. The dependent variable is payouts to assets ratio in Column 5, EBIT to assets ratio in Column 6, the log of firm i's market value in Column 7, and the log of firm i's stock price in Column 8. Firm-level controls include the log of firm i's total assets. The bottom of the table provides information about fixed-effects, and the level of clustering. Standard errors correct for clustering at the firm-level, and Standard errors are reported in parentheses. ***, ** and * indicate statistical difference from zero at the 1%, 5% and 10% levels, respectively. The ratios $Debt/Assets$, $Net\,Debt/Assets$, $Long-term\,Debt/Assets$, $Investment/Assets$, $Payouts/Assets$ and $EBIT/Assets$ are winsorized at the 1% level to minimize the influence of outliers. The results are robust to winsorizing at levels 0.001%, and 0.05%. Sources: Dealscan and Compustat (1990-2017).

Figure 1: Regulatory treatment of corporate exposures

Credit Assessment	AAA to AA-	A+ to A-	BBB+ to BB-	Below BB-	Unrated
Risk Weight	20%	50%	100%	150%	100%

Source: Basel II: International Convergence of Capital Measurement and Capital Standards: A Revised Framework - Comprehensive Version (2006), p.23

Figure 2: Regulatory treatment of securitisation exposures

External Rating	Basel Risk Weights
AAA	7%
AA	8%
A+	10%
A	12%
A-	20%
BBB+	35%
BBB	60%
BBB-	100%
BB+	250%
BB	425%
BB-	650%
Below BB- and unrated	Deduction

Source: Basel II: International Convergence of Capital Measurement and Capital Standards: A Revised Framework - Comprehensive Version (2006), p.135

Figure 3: Countries that adopted risk-sensitive capital (2004-2014)

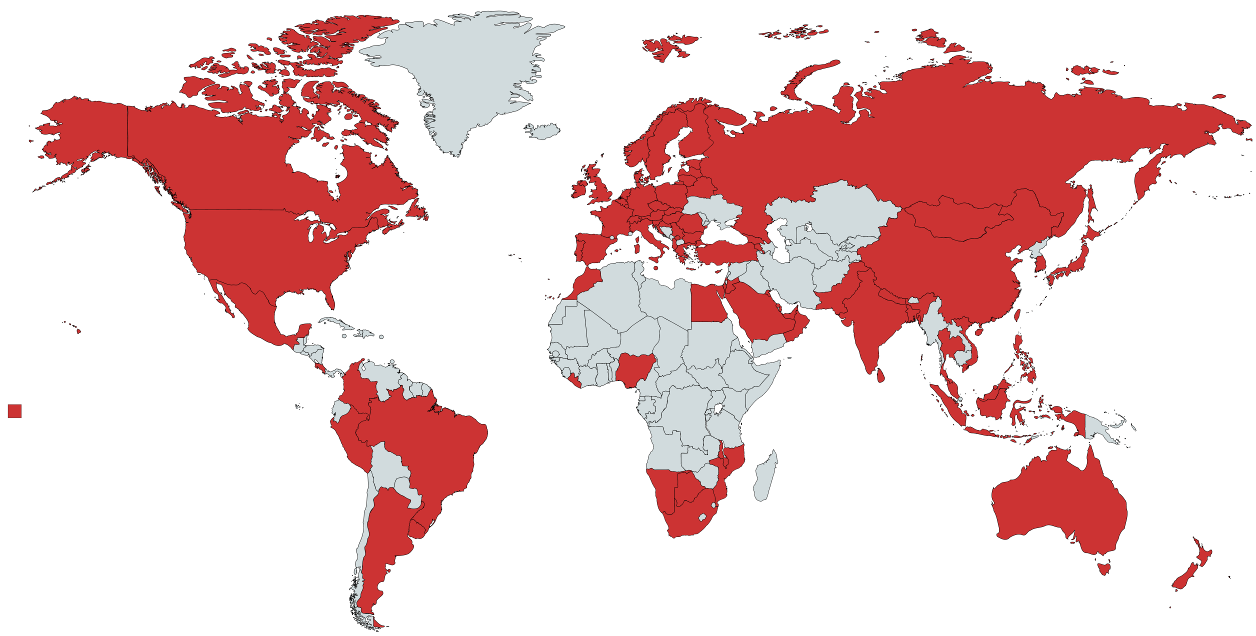
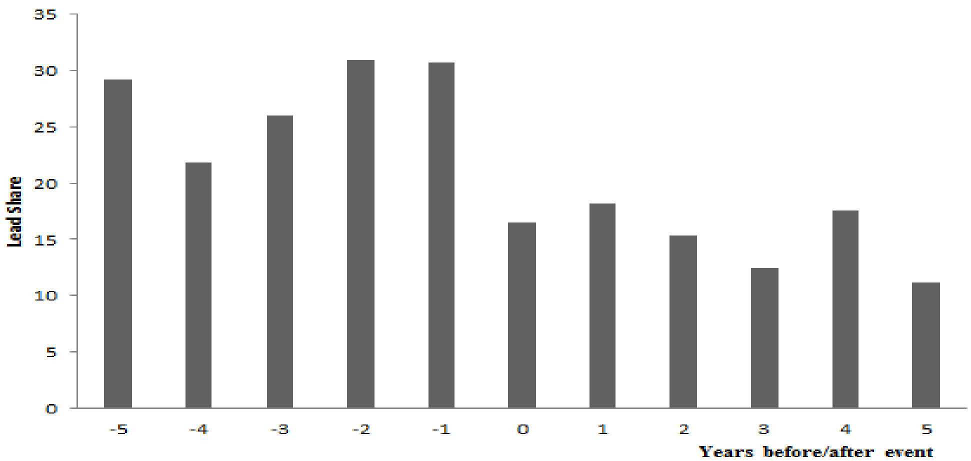
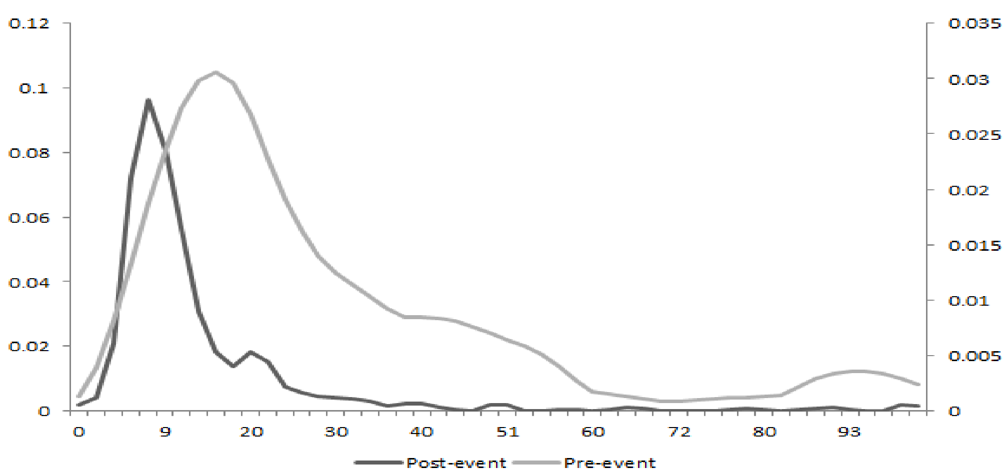


Figure 4: Lead Share in Speculative-grade Deals around Risk-Sensitive Capital



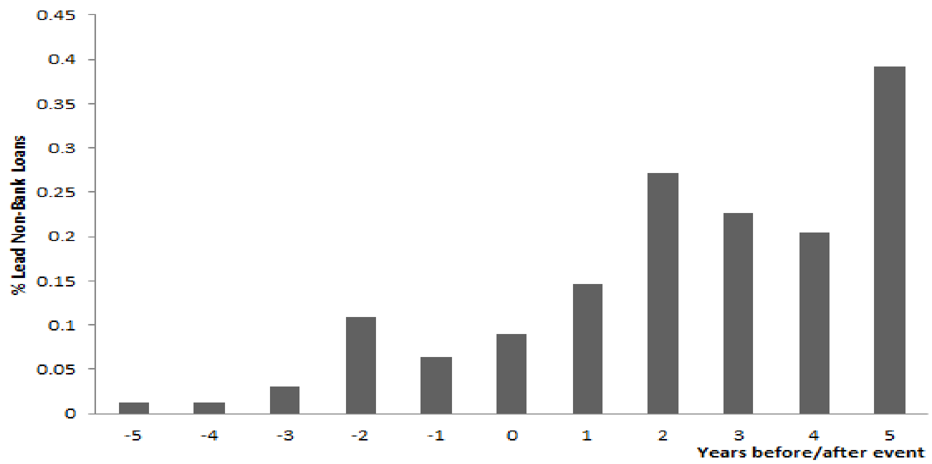
This figure shows the share of loans that lead banks allocate to a given facility for speculative-grade loans originated by the U.S. subsidiaries of foreign banks that become subject to the risk-sensitive capital regulation during the sample period. To compute the lead shares on the y-axis, I align contracts around the year of risk-sensitive capital regulation and calculate the average lead bank share for all contracts with the same distance to the regulation. The x-axis displays the distance to lead banks' exposure to the regulation in years. The value zero indicates the year in which the lead bank becomes subject to the regulation. Sources: Dealscan (1990-2017).

Figure 5: Lead Share Distribution around Risk-sensitive Capital



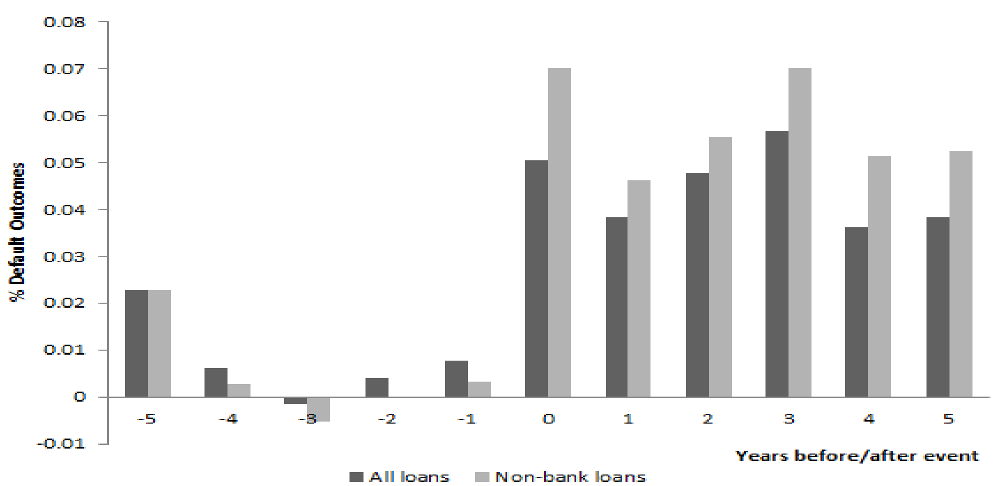
This figure depicts the kernel density plots of the share of loans that lead banks allocate to a given facility for speculative-grade loans originated by the U.S. subsidiaries of foreign banks that become subject to risk-sensitive capital during the sample period before the regulation (grey line) and after the regulation (black line).

Figure 6: Change in Non-Bank Lead Participation around Risk-sensitive Capital



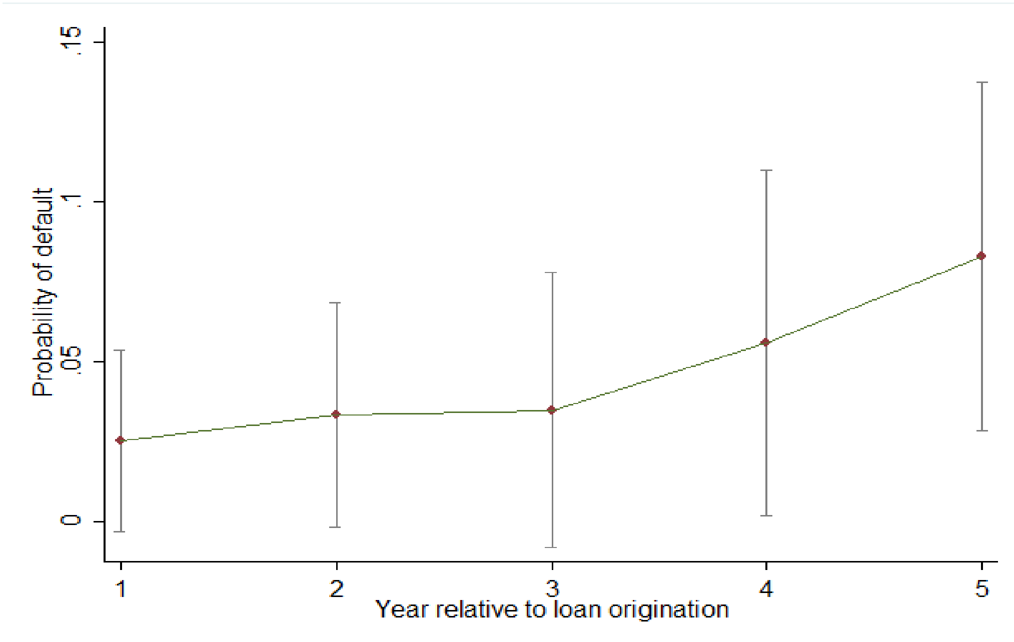
This figure shows the probability of having a non-bank lead arranger for speculative-grade loans originated by the U.S. subsidiaries of foreign banks that adopt the risk-sensitive capital regulation during the sample period. To compute the % of lead non-bank loans on the y-axis, I align contracts around the year of risk-sensitive capital regulation and calculate the share of loans with a non-bank laed arranger for all contracts with the same distance to the regulation. The x-axis displays the distance to lead banks' exposure to the regulation in years. The value zero indicates the year in which the lead bank becomes subject to the regulation. Sources: Dealscan (1990-2017).

Figure 7: Change in *Ex post* Borrower Default around Risk-sensitive Capital



This figure shows the probability of *ex post* borrower defaults for speculative-grade loans originated by the U.S. subsidiaries of foreign banks that adopt the risk-sensitive capital regulation during the sample period. To compute the % of default outcomes on the y-axis, I align contracts around the year of risk-sensitive capital regulation and calculate the average market-adjusted *ex post* borrower default for all contracts with the same distance to the regulation. The x-axis displays the distance to lead banks' exposure to the regulation in years. The value zero indicates the year in which the lead bank becomes subject to the regulation. Sources: Dealscan (1990-2013) and Moody's DRD (1990-2017).

Figure 8: Different Default Windows



This figure shows the coefficient estimate on Equation 3 and 95% CI using *ex post* default outcomes with different default windows after loan origination.

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Appendix to "The Shadow Disintermediation of Risk-sensitive Capital"

Irem Erten

LBS

This appendix has two sections. Section A contains additional information on descriptive statistics and variable definitions. Section B presents additional results and robustness checks.

A Descriptive Statistics and Variable Definitions

A.1 Descriptive Statistics

Table A1 reports the summary statistics. Panel A presents the event statistics, and Panel B depicts the summary statistics on the speculative-grade loan sample. In Panel B, the average lead share is 25.87% and the average interest rate on the loan is 281.51 basis points. Speculative-grade loans have, on average, a maturity of 60 months (5 years) and around two syndicate participants at loan origination. The average default rate within a five-year window after taking out a speculative-grade loan is 16%, and the average bankruptcy rate is 13%. On average, 17% of speculative-grade loans have a non-bank lead arranger and 77% of them have a non-bank participant or a non-bank term tranche. Panel C presents the Compustat sample[14].

[Table A1 here]

A.2 Variables

Table A2 reports the variable definitions.

[Table A2 here]

[14] I require that there exist at least one loan in Dealscan to the company

Table A1: Descriptive Statistics

Panel A: Event Statistics					
Event Year	# Affected Banks	# Total Banks	# Treated deals	# Total deals	# Treated/# Total deals
2004	9	187	31	511	0.06
2005	20	155	162	486	0.33
2006	12	161	160	448	0.36
2007	16	154	181	442	0.41
2008	17	153	79	167	0.47
2009	7	130	74	202	0.37
2010	8	156	151	286	0.53
2011	11	158	165	354	0.47
2012	6	151	153	267	0.57
2013	8	171	230	358	0.64
2014	6	168	156	259	0.60
Panel B: Loan-Level Data					
Variable	Mean	Min	Max	Obs.	Data Source
Lead share:	25.87	0	100	3560	Dealscan
Allindrawn (max. in a package):	281.51	5	1500	6742	Dealscan
# Total lenders:	1.98	1	19	6947	Dealscan
Default rate:	0.16	0	1	6600	Moody's
Bankruptcy rate:	0.13	0	1	6600	Moody's
Maturity (months):	60	1	301	6823	Dealscan
Non-bank lead:	0.17	0	1	6947	Dealscan
Non-bank institutional loan:	0.77	0	1	6947	Dealscan
Panel C: Firm-Level Data					
Variable	Mean	Median	Std. Dev.	Obs.	Data Source
Total assets (in millions)	10991.32	448.75	89467.30	144267	Compustat
Total debt/assets	0.21	0.26	0.26	80413	Compustat
Long-term debt/assets	0.23	0.18	0.23	143739	Compustat
Investment/assets	0.06	0.04	0.07	133963	Compustat
EBIT/assets	0.02	0.07	0.23	136036	Compustat
Cash/assets	0.10	0.04	0.13	141201	Compustat
Closing stock price	32.73	14.50	897.65	128686	Compustat

Panel A provides information on the event, including the number of lead banks that become subject to risk-sensitive capital regulation each year, total number of lead banks in Dealscan, the number of speculative-grade deals originated by affected banks, the total number of speculative-grade deals, and the ratio of speculative-grade deals originated by affected banks to the total number of speculative-grade deals. Panel B depicts the loan-level data, which comprises lead share, interest rate (allindrawn), # total lenders (total number of participants in the deal at loan origination), default (a dummy variable that takes one if the borrower defaults within five years of taking out the loan), bankruptcy (a dummy variable that is one if the borrower goes bankrupt within five years of taking out the loan), maturity of the deal in months, non-bank lead (a dummy that is one if deal has a non-bank lead arranger), and non-bank institutional loan (a dummy that takes one if loan is either originated by non-banks or has a non-bank term tranche). Panel C depicts the Compustat data for the firms which appear in Dealscan at least once during the sample period. Sources: Dealscan, Compustat, and Moody's Default & Recovery Database (1990-2017).

Table A2: Definitions of Variables

Variable	Units	Description	Source
Lead arranger	Dummy	Dummy that is one if lender has lead arranger credit based on Reuters LPC's League Table guidelines (LeadArrangerCredit)	Dealscan
Lead share	Decimal	Amount that lead lender committed to a given facility (BankAllocation)	Dealscan
allindrawn	Decimal	Amount that borrower pays in basis points over LIBOR for each dollar drawn down, including the fees (AllInDrawn)	Dealscan
Non-bank lender	Dummy	Dummy that is one if lender is defined as "Corporation", "Leasing Company", "Finance Company", "Mutual Fund", "Pension Fund", "Specialty", "Inst. Invest. CDO", "Inst. Invest. Other", "Inst. Invest. Hedge Fd", "Inst. Invest. Prime Fd", "Distressed (Vulture) Fund", "Inst. Invest. Insur. Co.", "Insurance Company", "Project", "Other", and zero otherwise	Dealscan
Non-bank lead	Dummy	Dummy variable that equals one if deal has a non-bank lead arranger	Dealscan
Non-bank term loan	Dummy	Dummy variable that is one if deal is classified as Term Loan or Term Loan with higher-order designations than A	Dealscan
Non-bank participant	Dummy	Dummy variable that equals one if deal has a non-bank participant at loan origination	Dealscan
Bank-only loan	Dummy	Dummy variable that takes on the value of one if deal neither has non-bank participants nor a non-bank term tranche	Dealscan
Non-bank institutional loan	Dummy	Dummy variable that is one if deal is either originated by a syndicate including non-banks or has a non-bank term tranche	Dealscan
# Total lenders	Number	Total number of participants in a deal	Dealscan
Maturity	Number	Maximum maturity of a facility in a deal (How long in months a facility will be active from signing to expiration date (maturity))	Dealscan
Bank maturity	Number	Maximum maturity of a bank tranche (*i.e.*, facility including bank participants) in a deal	Dealscan
Non-bank maturity	Number	Maximum maturity of a non-bank tranche (*i.e.*, facility including non-bank participants) in a deal	Dealscan
# Covenants	Number	Total number of covenants in a deal	Dealscan
Cov-lite	Dummy	Dummy variable that is one if loan has a facility classified as Covenant Lite by Dealscan	Dealscan
Dividend restrictions	Dummy	Dummy variable that is one if loan restricts borrower from paying shareholder dividends (DividendRestrictions)	Dealscan
Excess CF Sweep	Decimal	Percentage of net proceeds from excess CF that must be used to pay outstanding loans (ExcessCFSweep)	Dealscan
Asset Sales Sweep	Decimal	Percentage of net proceeds from asset sales that must be used to pay outstanding loans (AssetSalesSweep)	Dealscan
Debt Issuance Sweep	Decimal	Percentage of net proceeds from the issuance of debt that must be used to pay outstanding loans (DebtIssuanceSweep)	Dealscan
Equity Issuance Sweep	Decimal	Percentage of net proceeds from the issuance of equity that must be used to pay outstanding loans (EquityIssuanceSweep)	Dealscan
Insurance Proceeds Sweep	Decimal	Percentage of net proceeds from insurance settlements that must be used to pay outstanding loans (InsuranceProceedsSweep)	Dealscan
Default	Dummy	Dummy variable that is one if borrower experiences default within five years of taking out a loan.	Moody's Analytics Default & Recovery Database.
Bankrupt	Dummy	Dummy variable that is one if borrower experiences bankruptcy within five years of taking out a loan	Moody's Analytics Default & Recovery Database
Assets	Millions USD	Total Assets (at)	Compustat
Debt	Millions USD	Total debt including long-term and current debt (dt)	Compustat
Net Debt	Millions USD	Total debt (dt) minus cash (che)	Compustat
Long-term Debt	Millions USD	Total long-term debt (dltt)	Compustat
dd1	Millions USD	Long-term debt due in one year (dd1)	Compustat
dd2	Millions USD	Long-term debt due in two years (dd2)	Compustat
dd3	Millions USD	Long-term debt due in three years (dd3)	Compustat
dd4	Millions USD	Long-term debt due in four years (dd4)	Compustat
dd5	Millions USD	Long-term debt due in five years (dd5)	Compustat
Cash	Millions USD	Cash and short-term investments (che)	Compustat
Investment	Millions USD	Capital Expenditures (capx)	Compustat
Payouts	Millions USD	Total Dividends (dvt)	Compustat
Employment	Millions USD	Number of employees (emp)	Compustat
Current Assets	Millions USD	Total current assets (act)	Compustat
Current Liabilities	Millions USD	Total current liabilities (lct)	Compustat
WC	Millions USD	Working capital, calculated as current assets (act) minus current liabilities (act)	Compustat
EBIT	Millions USD	Earnings before interest and taxes (ebit)	Compustat
Stock Price	USD	Annual fiscal closing price ($prcc_f$)	Compustat
Shares Outstanding	Number	Common shares outstanding at year-end (csho)	Compustat
Market Value	Millions USD	Closing price multiplied by shares outstanding ($prcc_f * csho$)	Compustat
Tobin's Q	Ratio	The ratio of market value ($prcc_f * csho$) to assets (at)	Compustat
Share Ownership	Ratio	Percentage of total shares owned by firm's CEO (shrown tot pct)	Execucomp
Termination payments	Thousands USD	Estimated payments in event of involuntary termination (term pymt)	Execucomp
Change in control payments	Thousands USD	Estimated payments in event of change in control (chg ctrl pymt)	Execucomp
Severance pay	Dummy	Dummy variable that equals one if firm's CEO has positive estimated termination or change in control payments	Execucomp

B Additional Results

B.1 Hazard model: Adoption of the rules

One concern is that countries adopt risk-sensitive capital requirements when banks relax lending standards due to macroeconomic conditions. If banks relax lending standards on a consolidated basis, the impact of the regulation on U.S. subsidiaries may be endogenous. In this part of my analysis, I use a hazard model to rule out this concern.

I first include variables such as inflation and GDP growth to see whether macroeconomic factors predict the adoption decision. Macroeconomic conditions might matter since in economic booms, banks might lower lending standards during booms due to improved borrowers' future income and collateral prospects that increases their risk tolerance. Next, I include variables that capture the level of economic development — such as GDP per capita, population size, population growth, and the size of domestic credit market by banks. Because risk-sensitive capital requires the adoption of an extensive risk management system that has to be certified by the regulator, it imposes a significant compliance cost on both regulators and banks. Thus, countries with a large banking sector and a high level of economic development are more likely to welcome such regulations. I also add proxies that capture the level of concentration in the banking sector. Large incumbents could lobby for the regulation because it reduces overall capital requirements for large banks – generating a competitive advantage (Behn, Haselmann and Vig, 2016). Finally, I include the average level of bank capitalization. If banks are under-capitalized, countries might introduce capital rules to require banks to improve their risk management and to become better capitalized.

Table B1 presents results from the Cox proportional hazard model. The methodology does not impose any structure on the baseline hazard rate, and it calculates relative hazard rates for one-unit changes in the right-hand-side variables. As exhibited in Column (1), inflation and GDP per capita growth cannot predict the adoption decision. In Column (2), I find that population size and growth do not seem to matter either. In Column (3), however, I show that GDP per capita has a positive and statistically significant impact. In fact, a unit change in GDP per capita increases the relative hazard ratio by 0.7%. Furthermore, a unit change in the size of the domestic credit market increases the relative hazard ratio by 1%. In Column (4) and (5), I find concentration and capitalization in the banking sector do not play a significant role in the adoption of the regulation. In Column (6), however, the coefficient on GDP per capita is positive but no longer significant. Thus, the only factor that is consistently significant and economically meaningful is the size of the domestic market for bank credit. Overall, the hazard model supports that risk-sensitive capital regulation is exogenous to the U.S. subsidiaries: it is not correlated with macroeconomic conditions that coincide with the lending standards of the parent banks. It is instead linked to the size of the domestic credit market – a persistent characteristic of the home economies.

[Table B1 here]

Table B1: Hazard Model - Determinants of the adoption decision

	(1)	(2)	(3)	(4)	(5)	(6)
GDP per capita growth	-0.03093					-0.01172
	(0.01977)					(0.03476)
Inflation	-0.02499					0.00124
	(0.02212)					(0.02346)
Population (in millions)		0.00009				-0.00025
		(0.00034)				(0.00052)
Population growth		0.02666				0.04525
		(0.03246)				(0.04392)
Private credit to GDP by banks			0.00782***			0.01004***
			(0.00254)			(0.00294)
GDP per capita (in thousands)			0.01139**			0.00690
			(0.00523)			(0.00646)
Average bank capital ratio				0.00020		-0.00133
				(0.00672)		(0.00667)
Concentration					0.00345	-0.00145
					(0.00539)	(0.00522)
Observations	1390	1685	1204	1690	951	731
Log-likelihood	-309.361	-345.161	-295.831	-345.268	-295.539	-259.385
p-value of chi2 for regression	0.042	0.003	0.000	0.002	0.000	0.000
Region FE	Yes	Yes	Yes	Yes	Yes	Yes

This table shows the results from the Cox proportional hazard model to understand the decision to adopt the risk-sensitive capital regulation. The coefficients represent the relative hazard rates for one-unit changes in the independent variables. Standard errors correct for clustering at the country-level, and they are reported in parentheses. ***, ** and * indicate statistical difference from zero at the 1%, 5% and 10% levels, respectively. Sources: World Bank, Bankscope and FRED (1990-2015).

B.2 Do shadow banks learn from adverse selection?

In this section, I investigate whether shadow banks learn over time from adverse selection vis-à-vis lead arrangers. To do so, I test whether lead arrangers with a high rate of historical defaults become less likely to distribute speculative-grade loans to shadow banks. The results are collected in Table B2. In Column 1, I show that a lead arranger with a high track record of borrower defaults (greater than 10%) within the last five years becomes 10% less likely to sell loans to non-banks in the secondary loan markets. The impact is negative and statistically significant at the 5% level. In Column 2, I find that lead banks with a track record of high borrower defaults is 4.84% less likely to syndicate a loan to non-banks in the primary loan markets. However, the effect is not statistically significant. In Column 3, I show that banks with high borrower defaults do not become less likely to co-originate loans with non-banks. These results are consistent with the view that banks whose borrowers have a high historical rate of default *ex post* experience reputational effects towards non-banks in the secondary loan markets. Thus, adverse selection vis-à-vis shadow banks is likely to be unwillingly internalized by secondary loan market non-bank participants that do not play any monitoring role.

[Table B2 here]

B.3 Do shadow banks have additional motives?

In this section, I exploit lender type heterogeneity to understand whether shadow banks invested in risky loans due to additional motives. For example, participating in the syndicated loan market might enable lenders to gain access to deal flow or other sources of revenue (Benmelech Dlugosz, and Ivashina, 2012). If this is true, then defaults should be higher for shadow banks that are more likely to have additional motives. Insurance companies, for example, might be willing to accept higher risk because investing in syndicated loans could lead to fees from cross-selling other products. Similarly, hedge funds and mutual funds might be willing to pay a discount because they could use the private information obtained in the loan market to trade in other securities (Ivashina and Sun, 2011). To test for additional motives, I interact $event_{jt}$ with a dummy variable that takes the value of one if $loan_{ijt}$ has an institutional investor of a specific type. The results are collected in Table B3.

Table B2: Non-bank response to adverse selection

	(1)	(2)	(3)
Dep. Var.:	*non-bank term loan*$_{ijt}$	*non-bank participant*$_{ijt}$	*non-bank lead*$_{ijt}$
high historical defaults$_{jt}$	-0.1069**	-0.0484	0.0219
	(0.0440)	(0.0377)	(0.0216)
Observations	5087	5087	5087
R-squared	0.263	0.269	0.570
Year FE	Yes	Yes	Yes
Lender FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Borrower State FE	Yes	Yes	Yes
Lender State FE	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes
Cluster	Lender	Lender	Lender

This table tests whether lead banks with a high track record of *ex post* borrower defaults become less likely to distribute speculative-grade loans to shadow banks. In Columns 1 to 3, the independent variable *high historical defaults*$_{jt}$ takes the value of one if more than 10% of the borrowers of bank j defaulted within the last five years. In Column 1, the dependent variable *non-bank term loan*$_{ijt}$ is one if loan ijt is sold to non-banks in the secondary loan markets. The variable *non-bank participant* is one if loan ijt has a non-bank participant at loan origination. The variable *non-bank lead*$_{ijt}$ takes the value of one if the loan contract ijt is co-originated with a non-bank lead arranger in the syndicate at loan origination. Controls include the log of firm i's sales (defined by Dealscan as sales at close), and dummies for the type of loan, state of firm i, industry of firm i, state of lender j, and parent country of lender j. The bottom of the table provides information about fixed-effects, and the level of clustering. Standard errors correct for clustering at the lender-level, and are reported in parentheses. ***, ** and * indicate statistical difference from zero at the 1%, 5% and 10% levels, respectively. Sources: Dealscan and DRD (1990-2017).

[Table B3 here]

I find no evidence that the affected speculative-grade loans distributed to insurance companies are more likely to default (column 5), which does not support the view that insurance companies accept higher credit risk to capture other businesses. Similarly, the results do not support the claim that hedge funds and mutual funds bear higher risk for motives such as access to private information. Instead, I find that loans allocated to hedge funds and mutual funds after the reform are not systematically riskier. While the difference in defaults for loans in which mutual funds invested is positive (Column 3), it is not significant. The loans distributed to hedge funds are 15.95 points less likely to lead to *ex post* default (Column 6).

On the other hand, loans distributed to finance companies are significantly more likely to exhibit defaults (Column 1). Finance companies typically specialize in extending credit to risky businesses and consumers. Thus, they are unlikely to invest in syndicated loans for other sources of revenue. Loans extended to investors classified as corporations by Dealscan are also 7.61 percentage points more likely to exhibit defaults (Column 2). This group of investors include non-financial technology and manufacturing firms such as General Electric Co, and General Motors, which are likely to be the non-bank type with the least monitoring expertise in corporate lending, in addition to investment management companies such as KKR & Co, Bain Capital Credit, and Golub Capital that are active CLO managers – with arguably weakest monitoring incentives (Benmelech Dlugosz, and Ivashina, 2012). Hence, I do not find evidence that shadow banks invested in riskier loans for motives such as access to other businesses. Instead, defaults are higher for the types of shadow banks that are most likely to be uninformed about loan quality. However, I cannot rule out the possibility that they willingly accept a discount on the loans to gain market share in syndicated lending.

B.4 Is the rise in default risk due to *ex post* execution?

The higher incidence of defaults after the regulation could be driven by the possibility that banks become systematically less capable of executing syndicated loan contracts (*e.g.*, less able to renegotiate effectively) under the new rules. To rule out this concern, I compare loans originated and maturing before the regulation, with the loans originated before, but maturing after the regulation. If loan execution is systematically worse after the regulation,

Table B3: Default risk – Non-bank Type

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var.:			$default_{ijt}$			
$event_{ijt}$	0.0430	0.0720***	0.0810***	0.0824***	0.0817***	0.0834***
	(0.0276)	(0.0273)	(0.0275)	(0.0274)	(0.0274)	(0.0277)
$event_{ijt}$*$finance\,company_{ijt}$	0.0761***					
	(0.0165)					
$event_{ijt}$*$corporation_{ijt}$		0.0514***				
		(0.0153)				
$event_{ijt}$*$mutual\,fund_{ijt}$			0.0982			
			(0.0631)			
$event_{ijt}$*$collateralized\,loan\,obligation_{ijt}$				0.0950		
				(0.1091)		
$event_{ijt}$*$insurance\,company_{ijt}$					0.0504	
					(0.0637)	
$event_{ijt}$*$hedge\,fund_{ijt}$						-0.1595***
						(0.0335)
Observations	4575	4575	4575	4575	4575	4575
R-squared	0.248	0.246	0.246	0.245	0.245	0.245
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Lender FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes	Yes	Yes
Lender Parent Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Borrower State FE	Yes	Yes	Yes	Yes	Yes	Yes
Lender State FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Lender	Lender	Lender	Lender	Lender	Lender

This table reports the results concerning default risk for loan contracts with different types of non-bank institutional investor involvement in the syndicate at loan origination. In Columns 1 to 6, the dependent variable $default_{ijt}$ takes the value of one if firm i defaults any time within five years of taking out the loan. The dummy variable $event_{jt}$ is one in the post-regulation period after bank j becomes subject to risk-sensitive capital regulation. The variable *finance company*$_{ijt}$ is one if loan ijt has a lender identified by Dealscan as "Finance Company" or "Leasing Company", and the variable *corporation*$_{ijt}$ has the value of one if loan ijt has a participant categorized by Dealscan as "Corporation". The variable *mutual fund*$_{ijt}$ takes the value of one if loan ijt has a lender identified by Dealscan as an "Inst. Invest. Prime Fd.", "Mutual Fund", or "Pension Fund". The variable *collateralized loan obligation*$_{ijt}$ takes the value of one if loan ijt has a participant categorized by Dealscan as "Inst. Invest. CDO". The variable *insurance company*$_{ijt}$ is one if loan ijt has a participant identified by Dealscan as "Inst. Invest. Insur. Co." or "Insurance Company". The variable *hedge fund*$_{ijt}$ has the value of one if loan ijt has a lender categorized by Dealscan as "Distressed (Vulture) Fund" and "Inst. Invest. Hedge Fd". Controls include the log of firm i's sales (defined by Dealscan as sales at close), and dummies for the type of loan, state of firm i, industry of firm i, state of lender j, and parent country of the lender j. The bottom of the table provides information about fixed-effects, and the level of clustering. Standard errors correct for clustering at the lender-level, and are reported in parentheses. ***, ** and * indicate statistical difference from zero at the 1%, 5% and 10% levels, respectively. Sources: Dealscan and DRD (1990-2013).

then borrower defaults should also rise for loans that were originated, but are executed under the new rules. The results are collected in Table B4, Columns 1–4. I observe no differences in the probability of default. Thus, the rise in default risk is not due to the lead bank's deal execution ability after the reform.

[Table B4 here]

Table B4: Default Risk: Execution

	(1)	(2)	(3)	(4)
	Speculative-grade		Non-speculative-grade	
Dep. Var.:	$default_{ijt}$	$bankrupt_{ijt}$	$default_{ijt}$	$bankrupt_{ijt}$
loan originated before the $event_{ijt}$	-0.0332	-0.0002	-0.0220	0.0138
	(0.1329)	(0.1344)	(0.1194)	(0.1096)
loan originated before but maturing after the $event_{ijt}$	-0.0272	-0.0756	-0.0642	-0.0645
	(0.1576)	(0.1475)	(0.1509)	(0.1411)
Observations	4575	4575	7005	7005
R-squared	0.244	0.255	0.221	0.225
Year FE	Yes	Yes	Yes	Yes
Lender FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes
Lender Parent Country FE	Yes	Yes	Yes	Yes
Borrower State FE	Yes	Yes	Yes	Yes
Lender State FE	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Cluster	Lender	Lender	Lender	Lender

This table reports the results concerning default risk using Equation 3. In Columns 1 and 2, the dependent variable $default_{ijt}$ takes the value of one if firm i defaults any time within five years of taking out the loan. In Columns 2 and 4, the dependent variable $bankrupt_{ijt}$ takes the value of one if the borrower goes bankrupt any time within five years of taking out a loan. The dummy variable $event_{jt}$ is one after bank j becomes subject to the risk-sensitive capital regulation. The dummy variable loan originated before the $event_{jt}$ is one if the loan is granted before the risk-sensitive capital regulation and zero otherwise. The dummy variable loan originated before but maturing after the $event_{jt}$ is one if the loan is granted before, but matures after the risk-sensitive capital regulation, and zero otherwise. Controls include the log of firm i's sales (defined by Dealscan as sales at close), and dummies for the type of loan, state of firm i, industry of firm i, state of lender j, and parent country of lender j. The bottom of the table provides information about fixed-effects, and the level of clustering. Standard errors correct for clustering at the lender-level, and are reported in parentheses. ***, ** and * indicate statistical difference from zero at the 1%, 5% and 10% levels, respectively. Sources: Dealscan and DRD (1990-2013).

B.5 Who bears the increase in default risk?

In this section, I provide a back-of-envelope test to understand who bears the increase in default risk. I first calculate in the old regime the average default risk of the lead bank in a high yield loan as 0.039 (default risk multiplied by loan share before the reform). In the new regime, the new default risk multiplied by the new loan share and the probability of extending the high yield loan yields 0.0342 (48 basis points lower than before the regulation). Bank tranches mature much earlier than shadow bank tranches (45 months as opposed to 65 months), and the increase in default risk for a four year-window is around 5%. However, the rise in default risk for a five-year window is 8%. The back-of-envelope tests imply that although banks originate riskier loans, they bear less risk due to loan sales. However, credit risk distributed to the shadow banking system significantly rises.

[Table B5 here]

Table B5: Default Risk Exposure

Lead Bank Exposure	Before	After
Default Risk	0.15	0.20
Loan Share	0.26	0.19
Default Risk Exposure	**0.039**	0.038
Default Risk Exposure*P(High Yield Loan)		**0.0342**
Shadow Bank Exposure	Before	After
Default Risk	**0.15**	0.23
Default Risk*P(High Yield Loan)		**0.207**

B.6 Robustness

In this section, I formally test for pre-trends in the increase in default probability after the regulation. To do so, I define the dummy variables $event_{j*t-1}$, $event_{j*t-2}$, and $event_{j*t-3}$, that take the value of one at one, two and three years prior to the regulation, respectively. I find that all these coefficients are insignificant at a 10% level. Hence, there is no evidence of pre-trends. I present these results in Table B6.

[Table B6 here]

Another concern is that I exploit cross-sectional variation in incentives and information – which likely correlate with other time-varying confounding factors. To mitigate this concern, I run separate tests in which I interact the event dummy with other candidate confounds such as time-varying state, industry and size trends. Results are presented in Table B7. The estimate of interest ranges between 0.24 and 0.27 and it is only slightly affected.

[Table B7 here]

Table B6: Default Risk Pre-trends

	(1)	(2)
Dep. Var.:	$default_{ijt}$	$bankrupt_{ijt}$
$event_j *_{t-3}$	0.0318	0.0258
	(0.0631)	(0.0528)
$event_j *_{t-2}$	0.0421	0.0176
	(0.0456)	(0.0316)
$event_j *_{t-1}$	-0.0332	-0.0405
	(0.0589)	(0.0381)
$event_{jt}$	0.0857***	0.0699**
	(0.0315)	(0.0292)
Observations	4575	4575
R-squared	0.246	0.257
Year FE	Yes	Yes
Industry FE	Yes	Yes
Loan Purpose FE	Yes	Yes
Lender Parent Country FE	Yes	Yes
Borrower State FE	Yes	Yes
Lender State FE	Yes	Yes
Firm Controls	Yes	Yes
Cluster	Lender	Lender

This table reports the results concerning default risk using from Equation 3. In Columns 1, the dependent variable $default_{ijt}$ takes the value of one if firm i defaults any time within five years of taking out the loan. In Columns 2, the dependent variable $bankrupt_{ijt}$ takes one if the borrower goes bankrupt within five years of taking out a loan. The dummy variable $event_{jt}$ is one after bank j becomes subject to the risk-sensitive capital regulation. The dummy variables $event_{j*t-1}$, $event_{j*t-2}$, and $event_{j*t-3}$ take the value of one at one, two and three years prior to the regulation, respectively. Controls include the log of firm i's sales (defined by Dealscan as sales at close), and dummies for the type of loan, state of firm i, industry of firm i, state of the lender j, and parent country of lender j. The bottom of the table provides information about fixed-effects, and the level of clustering. Standard errors correct for clustering at the lender-level, and are reported in parentheses. ***, ** and * indicate statistical difference from zero at the 1%, 5% and 10% levels, respectively. Sources: Dealscan and DRD (1990-2013).

Table B7: Lead Share Robustness

	(1)	(2)	(3)	(4)	(5)	(6)
			$default_{ijt}$			
$treated_j$	-0.0399	-0.0397	-0.0423*	-0.0404	-0.0529**	-0.0360
	(0.0249)	(0.0249)	(0.0249)	(0.0250)	(0.0253)	(0.0240)
$event_{jt}$	0.1612	0.1579***	-0.0227	0.0801***	0.0805***	0.0750***
	(0.3486)	(0.0430)	(0.0292)	(0.0280)	(0.0286)	(0.0283)
$event_{jt}$*$low\, lead\, share_{jt}$	0.2512***	0.2350***	0.2322***	0.2490***	0.2693***	0.2706***
	(0.0563)	(0.0544)	(0.0578)	(0.0564)	(0.0744)	(0.0529)
$low\, lead\, share_{jt}$	-0.0503	-0.0511	-0.0496	-0.0498	-0.0452	-0.0556
	(0.0463)	(0.0464)	(0.0456)	(0.0464)	(0.0486)	(0.0465)
$treated_j$*$low\, lead\, share_{jt}$	-0.1390**	-0.1366**	-0.1286**	-0.1394**	-0.1226*	-0.1430**
	(0.0604)	(0.0608)	(0.0590)	(0.0606)	(0.0732)	(0.0586)
Observations	4575	4575	4575	4575	4575	4575
R-squared	0.246	0.248	0.250	0.246	0.206	0.216
Size - Event FE	Yes	No	No	No	No	No
Industry - Event FE	No	Yes	No	No	No	No
State - Event FE	No	No	Yes	No	No	No
Size - Year FE	No	No	No	Yes	No	No
Industry - Year FE	No	No	No	No	Yes	No
State - Year FE	No	No	No	No	No	Yes
Industry FE	Yes	No	Yes	Yes	No	Yes
Borrower State FE	Yes	Yes	No	Yes	Yes	No
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Lender FE	Yes	Yes	Yes	Yes	Yes	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes	Yes	Yes
Lender Parent Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Lender State FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Lender	Lender	Lender	Lender	Lender	Lender

This table adds time-varying trends to default risk for loans with different levels of lead bank incentives at loan origination. The dependent variable $default_{ijt}$ takes the value of one if firm i defaults any time within five years of taking out the loan. The dummy variable $event_{jt}$ is one after bank j becomes subject to the risk-sensitive capital regulation. The variable *low lead share*$_{ijt}$ takes one if lead arranger j has less than 5% participation in loan ijt at loan origination and zero otherwise. The bottom of the table provides information about fixed-effects, and the level of clustering. Standard errors correct for clustering at the lender-level, and are reported in parentheses. ***, ** and * indicate statistical difference from zero at the 1%, 5% and 10% levels, respectively. Sources: Dealscan and DRD (1990-2013).